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SEAN WILENTZ, recipient of the 1984 Beveridge Prize of the American Historical Association, is Assistant Professor and Philip and Beulah Rollins Preceptor in History at Princeton University. He was awarded the Theron Rockwell Field Prize and the George Washington Eggleston Prize, 1980, for the doctoral dissertation which formed the basis of this book. In 1983 he was awarded the first Edward Gaylord Bourne Medal for excellence in historical studies by Yale University.

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The American Historical Association, 1884–1984: Retrospect and Prospect

ARTHUR S. LINK

THE AMERICAN HISTORICAL ASSOCIATION is now slightly more than one hundred years old. Centennials do not happen very often, and tonight we can, with justifiable pride, review the past century and note the central role that our organization has played in the development of a large and many-faceted historical enterprise in the United States over the past century. And we can, even more appropriately, take a candid look at our association as it stands today and consider its problems, challenges, and opportunities.

It seems safe to say that some kind of national American historical association was inevitable during the last quarter of the nineteenth century. The original proposal for the association came from Daniel Coit Gilman, organizer and president of The Johns Hopkins University, the first modern research-oriented university in the United States.¹ But Herbert Baxter Adams, an associate professor of history at the Johns Hopkins (whom Woodrow Wilson later described as “a great Captain of Industry,—a captain in the field of systematic and organized scholarship”),² took the lead in planning the first meeting. Undoubtedly Adams drafted the call for the formation of the association at Saratoga Springs, New York, on September 9, 1884, a summons that was widely circulated.³ Before that date, Adams made certain that most of the leading historians in the country would attend, arranged the first program, and drew up the first slate of officers.⁴

On the morning of September 9, 1884, “a private gathering of the friends of the Historical Association,” as Adams called it, met in a small parlor of the United States Hotel in Saratoga Springs. The group discussed whether the new organization

I take this opportunity to thank Samuel R. Gammon, Eileen Gaylard, David W. Hirst, Manfred Boemeke, William A. Link, and, particularly, Richard W. Leopold for invaluable help while I was writing this paper.

¹ Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977), 168–71. For corroboration of Gilman's authorship of the idea, see J. Franklin Jameson, “The American Historical Association, 1884–1909,” *AHR*, 15 (1909–10): 4.

² Wilson to Richard T. Ely, January 30, 1902, in Arthur S. Link *et al.*, eds., *The Papers of Woodrow Wilson*, 48 vols. to date (Princeton, N.J., 1966–), 12: 264.

³ *Papers of the American Historical Association* [hereafter, *AHA Papers*], 1 (New York, 1886): 5.

⁴ Adams to D. C. Gilman, August 8, 1884, in W. Stull Holt, ed., *Historical Scholarship in the United States, 1876–1901: As Revealed in the Correspondence of Herbert B. Adams* (Baltimore, 1938), 71–72.

should remain a section of the American Social Science Association, founded in 1865, which included historians in its membership, or become an independent body. The answer was foreordained. Most of those present were full-time historians or economists; indeed, the group included leaders of those two fields, as well as political scientists. As the chairman of the group, Justin Winsor, librarian of Harvard College and editor of the then-new *Narrative and Critical History of America* series, argued: "We have come, gentlemen, to organize a new society, and fill a new field. . . . Our proposed name, though American by title, is not intended to confine our observations to this continent. We are to be simply American students devoting ourselves to historical subjects, without limitation in time or place. . . . We are drawn together because we believe there is a new spirit of research abroad,—a spirit which emulates the laboratory work of the naturalists, using that word in its broadest sense. That spirit requires for its sustenance mutual recognition and suggestion among its devotees."⁵

There followed a spirited discussion of a resolution to test the sentiment of the group, and only John Eaton, president of the ASSA and also United States Commissioner of Education, opposed the motion for independence. Later that same day, September 9, forty persons met in Putnam Hall and heard an address by the AHA's first president, Andrew D. White, a German-trained former professor of history at the University of Michigan and, since 1868, president of Cornell University. The association met in business session on the morning of September 10 and adopted a constitution presented by Charles Kendall Adams of the University of Michigan. Surely one of the shortest ever drafted, the constitution filled slightly more than a single printed page. The document said simply that the "name of this Society shall be the American Historical Association" and that "its object shall be the promotion of historical studies." The last two articles created officers and an Executive Council and provided a method of amendment. Any person approved by the Executive Council could become a member upon payment of annual dues of three dollars.⁶

If we can now see that the time was ripe for the organization of a national professional historical association, that fact was not self-evident in 1884. The universities and colleges in the entire United States appear to have had, as John Franklin Jameson later noted, only fifteen full-time professors of history and five full-time assistant professors. The number of graduate students in history was probably not more than thirty.⁷ Even so, the association began its second year with 220 members. They included a former president of the United States, Rutherford B. Hayes, who was one of the founders, and a future president of the United States and of the AHA (1924), who signed himself simply as "Woodrow Wilson, Esq., Johns Hopkins University, Baltimore, Md." In 1885, three of the members were women.

From this small beginning, the growth of the association during its first twenty-five years was phenomenal.⁸ By 1909 the AHA had a membership of about twenty-

⁵ *AHA Papers*, 1: 11.

⁶ *Ibid.*, 20–21.

⁷ Jameson, "The American Historical Association, 1884–1909," 2–3.

⁸ The growth was due in large part to an increase in the number of colleges and universities, an increase in

five hundred and was the largest and most active historical organization in the world. This growth occurred even while the association, which had originally included scholars in most branches of the social sciences, began to experience fragmentation owing to the professionalization of sister disciplines. The American Economic Association broke away in 1885, although it held joint meetings with the AHA in that year, in 1886, and in 1909. Then, after the founding of the American Political Science Association in 1903 and the American Sociological Society in 1905, each major social science discipline possessed its own national organization.

Constitutionally, the AHA underwent little change during the first ninety years of its existence. The original constitution entrusted governance to an Executive Council whose voting members included not only the elected members but also all past presidents. For example, in 1915, twenty-eight members of the Executive Council included twenty-one former presidents. In order to head off a revolt (which proved to be abortive) led by Frederic Bancroft (former librarian of the Department of State), Dunbar Rowland, and John H. Latané,⁹ the leaders of the Executive Council in 1915 put through an amendment, which decreed that presidents on leaving office might sit on the Council for life but could vote for only three years—"and no longer." An amendment adopted in 1969 permitted former presidents, who still held membership on the Council for life, to vote for only one year. The constitution adopted in 1974, under which we operate today, attempted to rationalize the work of what was now called simply the Council by reducing its elected membership from twelve to nine and by creating Research, Teaching, and Professional divisions, each headed by vice-presidents. The constitution of 1974 also excluded all former presidents from membership on the Council, with the exception of the current past president, who serves for one year following his or her incumbency.¹⁰

The American Historical Association has undergone many important structural changes since Herbert Baxter Adams conducted its business from his hip pocket. During its first decade, the AHA catered to nonacademic historians and public figures, including college and university presidents. Such a secretariat as it possessed was Adams's office in Baltimore. In 1889, A. Howard Clark of the National Museum, now a forgotten figure, was made assistant secretary and curator, a post he held until 1908. Being on the scene, he arranged for publication of the annual reports. Adams's death in 1901 compelled a reconsideration of the organization. Charles Homer Haskins was made secretary of the Executive Council and held that post until 1914. Clark was succeeded as secretary by Waldo Gifford Leland, who served until 1919. John Spencer Bassett and Dexter Perkins, neither

the enrollment in institutions of higher learning from 116,000 in 1880 to 355,000 in 1910, and, above all, to the important place that history occupied in college curricula. In addition, there was a steady growth in professional graduate study in history in the United States. In 1882, five universities in this country offered the Ph.D. degree in history and granted two persons that degree. By 1911–12, twenty-two American universities offered the Ph.D. in history and turned out annually twenty-seven recipients of that degree. By 1912, five hundred historians in the United States held the Ph.D. degree. Dexter Perkins *et al.*, *The Education of Historians in the United States* (New York, 1962), 16–17.

⁹ About this episode, see Ray Allen Billington, "Tempest in Clio's Teapot: The American Historical Association Rebellion of 1915," *AHR*, 77 (1973): 348–69.

¹⁰ For a review of these and other constitutional changes, see "The Evolution of the Constitution," *Perspectives*, 22 (May–June 1984): 16–20.

of them residents of Washington, succeeded as secretary in 1919 and 1928.

Actually, much of the continuity and organization was provided by Jameson, who edited the *American Historical Review* for most of the years from 1895 to 1928. When Jameson moved to Washington in 1905 to head the Bureau of Historical Research of the Carnegie Institution, he used the facilities of the institution as his editorial office. The AHA did not need new housing until 1928, when Jameson gave up the editorship of the *Review* and became chief of the Manuscript Division of the Library of Congress.

During the years 1928 to 1941 the AHA realized the need to concentrate its activities in Washington and obtain an executive secretary—full-time if possible, part-time if necessary. Conyers Read was the first in 1933, but he remained in Philadelphia and served only part-time. A constitutional amendment abolished the office of secretary, still held by Perkins. In 1941 Guy Stanton Ford became the first full-time executive secretary of the association and editor of the *Review*, with offices in what was then called the Library of Congress Annex. Concentration in Washington was achieved, and a building for the headquarters of the association was purchased in 1956. The combined editorial and executive functions became unworkable, however, because of the increase in the activities of the secretariat in the late 1950s. With Boyd Shafer's retirement in 1963–64, the inevitable split occurred. Succeeding executive secretaries and executive directors (the title changed under the constitution of 1974) were W. Stull Holt, 1963–64, who briefly recombined the offices of executive secretary and managing editor of the *AHR*; Louis B. Wright, 1964–65; Paul L. Ward, 1965–74; Mack Thompson, 1974–81; and Samuel R. Gammon, since 1981. The successors to Shafer as managing editor of the *American Historical Review* were Holt, 1963–64; Henry R. Winkler, 1964–68; and R. K. Webb, 1968–75. John Duffy, Robert F. Byrnes, and Robert E. Quirk were interim editors during the period when the *Review* was moved to Indiana University. Otto Pflanze assumed the editorship there beginning with the issue of April 1977.

LET US NOW TURN BACK to the character, ideals, and activities of the founders and members of the American Historical Association since its early years. The first thing to be said about the American Historical Association is that it has always been nondiscriminatory in its acceptance of members. The constitution of 1884 said simply, "Any person approved by the Executive Council may become a member by paying three dollars." Approval was always pro forma, if, indeed, it was actually ever given. And, to my knowledge, no person who has applied for membership in the AHA has been turned down on grounds of race, gender, religion, national origin, or political views.

Nonetheless, the association cannot boast of ever having been much in advance of the practices and customs of American society. Technically, a black woman could have been elected president. But the provisions of constitutions often do not conform to the realities of life. It is true that W. E. Burghardt Du Bois, the first black Ph.D. in the United States, was on the programs at AHA annual meetings in

1891 and 1909. In the annual reports and *American Historical Review*, the association also published his papers—the second of which was a major revisionist manifesto on Reconstruction¹¹—and a large number of papers on blacks in slavery and freedom. Yet few blacks were members of or active in the association until the 1960s, no black person was a member of the Council until 1959, and no black was president of the association until 1979.

Although the AHA had Jewish members from the beginning of the association, their numbers, until the 1950s, were infinitesimal compared to the total membership. In some extenuation, this was true because the AHA increasingly drew its members from the ranks of professional historians, and the unwritten but nearly ironclad rule against the employment of Jews in all except a few colleges and universities made it foolhardy for many Jewish undergraduates to contemplate a career in the historical profession. Anti-Semitic walls in the United States crumbled, however, with astonishing speed in the 1940s and 1950s, and Jews then began to enter the historical profession in large numbers and to play an increasingly active role as members and officers of the AHA. The first Jewish president served in 1953.

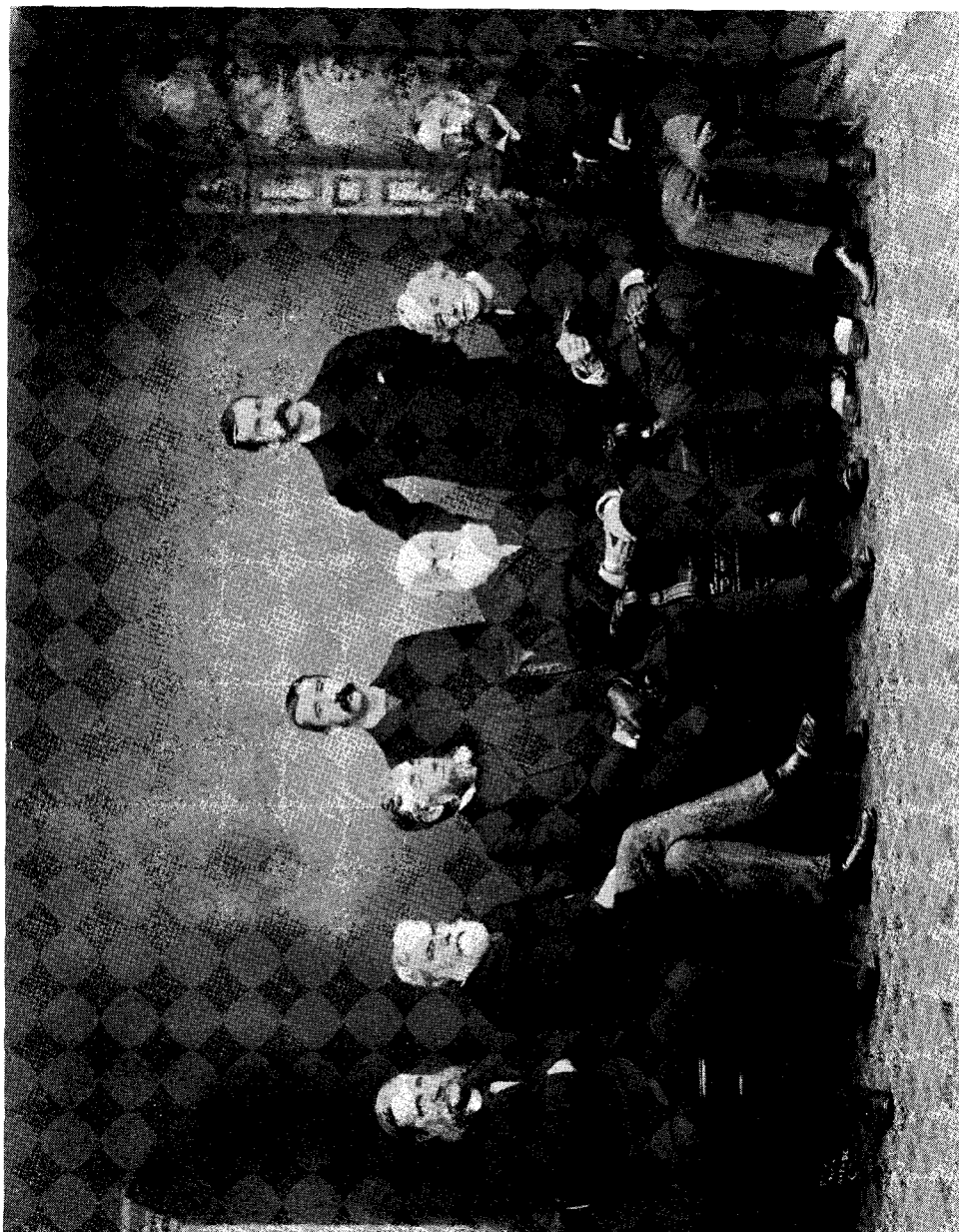
Women, too, have been members of the AHA since 1885. Indeed, a resolution adopted by the Executive Council at its first meeting proclaimed that, “in the opinion of the Council, there is nothing in the Constitution of the American Historical Association to prevent the admission of women into the Association upon the same qualifications as those required of men.”¹² Among 2,519 individual members in 1920, 483 women can be positively identified, or 19 percent of the whole. In 1950, 792 (about 15 percent) of 5,300 members were women. The AHA has kept membership lists and statistics on computer tapes since 1973. In that year, the association had some 16,000 dues-paying members, of whom 2,352 (about 15 percent) were women. In 1977, there were 10,620 male members and 2,774 female members (21 percent). In 1983, male membership stood at 8,691, female at 2,563 (23 percent).

Membership lists before 1960 reveal a number of distinguished female members of the AHA. Yet the records also reveal that women were given short shrift in positions of governance and leadership in the association for the greater part of that period. For example, out of a total of ninety-six members of the Executive Council before 1933, five were women, a situation that changed little during the next forty years. Only ten women served on the Executive Council between 1934 and 1971; during the same period, women had scattered representation on various committees, in a ratio of about one to nine. And the AHA has had only one woman president, who served in 1942.

Like all other professional organizations, the AHA could not withstand the winds of social change that have been gusting across the United States since the mid-1960s. For reasons at once too obvious and too complicated to discuss here, the

¹¹ Du Bois, “The Enforcement of the Slave Trade Laws,” *Annual Report of the American Historical Association* [hereafter, *Annual Report*], 1891 (Washington, 1892), 161–74, and “Reconstruction and Its Benefits,” *AHR*, 15 (1909–10): 781–99.

¹² *AHA Papers*, 1: 40.



Front row, left to right: William F. Poole, L.L.D., of Chicago; Justin Winsor, L.L.D., of Harvard University; Charles Kendall Adams, L.L.D., President of Cornell University; Hon. George Bancroft, L.L.D.; Hon. John Jay of New York; and Hon. Andrew D. White, L.L.D. *Back row, left to right:* Prof. Herbert B. Adams, Ph.D., of Johns Hopkins University; and Clarence Winthrop Bowen, Ph.D., Officers of the American Historical Association, photographed by Matthew Brady's studio, on December 30, 1889, on the occasion of the chartering of the association. The names, degrees, and affiliations are listed as on the original photograph. Photograph located by Katrina R. Pflanze and reproduced courtesy of Sterling Memorial Library, Yale University, New Haven, Conn.

barriers against broad and extensive participation and leadership by women in the AHA collapsed all at once—in 1973. In that year, three women sat on the Executive Council and on the nominating committee. And I think it accurate to say that since 1973 every nominating committee and every committee on committees has been cognizant of the great resources of scholarship and leadership that our female members bring to the association.

Growing diversity in the membership and leadership of the AHA since the 1950s has been the single most important event in the history of the association. It is a good thing that our founders declared that anyone could become a member of the AHA. It is a much better thing that the association, in approving the report of the so-called Hackney Commission in 1974, said that it welcomed and would honor, encourage, and defend as best it could all members without regard to gender, race, politics, religion, or lifestyle.¹³

PUBLICATION OF SCHOLARLY WORK has always been vital to the promotion of historical studies, and the American Historical Association since its founding has played a key, if not a dominant, role in this activity. The AHA's first reports, *Papers of the American Historical Association*, were published from 1885 to 1891 by G. P. Putnam's Sons of New York. Congress granted the association a federal charter in 1889 and said that the Smithsonian Institution might publish the annual reports. The first such publication was the annual report for 1889, published in 1890. And with publication and distribution free of cost, the annual reports almost immediately waxed huge in size. They included not only the reports of the annual meetings and the minutes of the Executive Council and standing and special committees but also, taken altogether, thousands of articles, monographs, and editions of letters and other documents. Publication on this scale continued until the late 1910s and then, after the mid-1920s, intermittently included editions of diaries, diplomatic documents, and the like.¹⁴ The *Annual Report* for 1945 was the last to contain edited collections, because the period 1945–50 marked a turning point in historical editing in the United States: the introduction of new scholarly standards and a widespread assumption that large, important scholarly editions had to be produced by groups of scholars collaborating on projects underwritten by foundations, corporations, and the federal government.

The spread of such series as the *Johns Hopkins University Studies in Historical and Political Science* (founded in 1882), the proliferation of specialized historical journals, such as the *Mississippi Valley Historical Review* (since 1964 the *Journal of American History*), and, probably most important, the rapid growth in the number of university presses in the 1920s and afterwards, all help account for the demise of the annual reports as outlets for monographs. Even so, the AHA did not abandon

¹³ American Historical Association, *Final Report Ad Hoc Committee on the Rights of Historians* (Washington, 1975).

¹⁴ For example, see Howard K. Beale, ed., *The Diary of Edward Bates, 1859–1866* (Washington, 1933); Bernard Mayo, ed., *Instructions to the British Ministers to the United States, 1791–1812* (Washington, 1941); Paul Knaplund, ed., *Letters from the Berlin Embassy, 1871–1874, 1880–1885* (Washington, 1944); and Grace Lee Nute, ed., *Calendar of the American Fur Company's Papers*, 2 vols. (Washington, 1945).

the field. By the late 1920s, it had the resources of the Albert J. Beveridge Fund, the Littleton-Griswold Fund, and what was called the revolving fund of the Carnegie Institution of Washington. With these resources it was able to sponsor and subsidize publication of many monographs in American history in general and, in the case of the Littleton-Griswold Fund, of early American legal records.

The founding of the *American Historical Review* by J. Franklin Jameson and others in 1895 brought into being another outlet for articles and, perhaps more important, a journal devoted to the critical review of historical works. The *AHR* was originally an independent journal, which was marketed commercially by the Macmillan Company; the editors, who legally owned the *Review*, gladly turned over title to and financial responsibilities for it to the association in 1915.¹⁵ Along with his board of editors, Jameson made the *AHR* a worthy rival of the *Historische Zeitschrift*, founded in 1876, and the *Revue historique*, founded in 1884. Under a series of distinguished editors, the *American Historical Review* has flourished and continues to be one of the few journals in the world that attempts to cover all fields of history. Since its founding, the *AHR* has always been the flagship of the association. May it ever continue to be thus!

Looking back over the works published in the annual reports and the *American Historical Review*, one is impressed most notably by their range and diversity. Since many early leaders of the AHA were trained by German professors who stressed the universality of history, and since this tradition was also strong in historical training in the United States, it was inevitable that the American Historical Association should have begun as an organization dedicated to the promotion of all historical study. It has been and continues to be the one historical organization in the United States that brings historians in all fields and specialties into discussion, communication, and fellowship. Therefore, it has been and continues to be the one and only organization that can speak for the entire historical profession in this country. Catholicity of interest is one of our noblest traditions. We must maintain it with all our strength in the century that lies ahead.

The thing that struck me most forcibly as I looked through the annual reports for the first twenty-five years was, first, the emphasis placed by the leaders of our profession on work in social, cultural, and economic history and on the experiences of so-called ordinary folk and, second, the degree to which young historians heeded the call of their mentors to write this kind of history. Andrew D. White's first presidential address, "On Studies in General History and the History of Civilization," was a clarion call for cultural history on a grand scale.¹⁶ In his presidential address of 1900, entitled "The New History," Edward Eggleston vented his scorn for Edward A. Freeman's famous aphorism, as follows: "Never was a falser thing said than that history is dead politics and politics living history. Some things are false and some things are perniciously false. This is one of the latter kind."¹⁷ James

¹⁵ Again, see Billington, "Tempest in Clio's Teapot."

¹⁶ *AHA Papers*, 1: 49-72.

¹⁷ *Annual Report, 1900* (Washington, 1901), 39. Even Herbert B. Adams, the leading proponent of institutional history in the United States who blazoned Edward A. Freeman's motto "History is Past Politics, and Politics are Present History" across the wall of the historical seminary at the Johns Hopkins, took pains to

Harvey Robinson had been speaking and writing about social and cultural history and the importance of the new social sciences to the study of history for years before he published his famous manifesto, *The New History*, in 1912. As John Higham has pointed out, the “new” history was not so new in 1912.¹⁸

As for methodology and styles and kinds of history, it can be said to their credit that the councils and committees of the AHA have always maintained an open mind on such matters without ever being coercive about them. By joining the American Council of Learned Societies upon its founding in 1919, the association affirmed that history is one of the humane disciplines; by joining the Social Science Research Council in 1925, after two years of hesitation, the association affirmed that history is also one of the social sciences. The association, of course, has had enthusiasts for various methodologies and interpretations but few sectarians, certainly not enough of them to divide the leadership and membership into warring camps. Even on the rare occasions when AHA committees have composed reports on methodology and historiography, they have written in good temper and with due respect for what might be called the great traditions of modern historical scholarship.¹⁹

The compilation of bibliographies consumed much of the energy of committees and members of the AHA during the first thirty-odd years of its existence. If our forebears seem to have had a passion for bibliographies, we must remember that they were in fact laying the foundations for historical research in this country. This work was formalized by the Executive Council's appointment of a Committee on Bibliography in 1898; it remained a standing committee of the Council until 1919. The most notable series sponsored by the association has been the publication in 1902–03, 1906–40, and 1948–61, of *Writings on American History*, compiled from 1906 through 1940 by Grace Gardner Griffin—that devoted servant of historians.²⁰ The crowning bibliographical achievement of the AHA was its *Guide to Historical Literature*, originally published in 1931 and drastically revised in 1961, when it was certainly the best single-volume bibliography of historical writing in all epochs and fields.

Limitations of time prevent a proper recognition of the work of the councils and committees of the AHA in what is called public history—in organizing local and state historical societies, cataloguing their resources, upgrading their standards, and encouraging the establishment of state historical commissions or state departments of archives and history. The Executive Council created a Public Archives Commis-

explain that Freeman had been badly misunderstood. “He used the word ‘political’ in a large Greek sense,” Adams wrote. “For him the Politeia or the Commonwealth embraced all the highest interests of man. He did not neglect the subjects of art and Literature.” Adams, “Is History Past Politics?” *Johns Hopkins Studies in Historical and Political Science*, 13th ser. (Baltimore, 1895), 190.

¹⁸ Higham et al., *History: Humanistic Scholarship in America* (Englewood Cliffs, N.J., 1965), 111.

¹⁹ See, for example, *Historical Scholarship in America: Needs and Opportunities, A Report by the Committee of the American Historical Association on the Planning of Research* (New York, 1932); Social Science Research Council, Bulletin 54, *Theory and Practice in Historical Study* (New York, 1946); and Michael Kammen, ed., *The Past Before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980).

²⁰ Since 1962 the AHA has continued this publication for articles; between 1962 and 1973 it published a parallel series for books on American history. There is a cumulative index for *Writings on American History*. The index and the successor series need to be put on computer tapes and made available to scholars by author, title, and subject.

sion in 1899, and that body began in 1909 to hold an annual conference of archivists. In 1904 the Executive Council also created a General Committee to hold annual conferences of persons active in local and state societies. Close cooperation between the AHA and local and state societies and archives continued until the establishment of the Society of American Archivists in 1936 and the American Association for State and Local History in 1940, which rendered the AHA's activities in these fields redundant.²¹

As historical scholars who insisted that work in original sources, particularly manuscripts, was the sine qua non of research, the first members of the AHA were appalled by the lack of guides to historical manuscripts in the United States. The business meeting of the association's second annual meeting instructed the Council "to represent to our government the advantages and the advisability of cataloguing all documents relating to the history of the United States down to the year 1800 existing in the official and private archives of Europe, and of copying and printing the more important of them."²² After the federal incorporation of the association in 1889, there were high hopes for the establishment of a federal historical manuscripts commission modeled on the British Historical Manuscripts Commission. In a paper entitled "The Expenditures of Foreign Governments in Behalf of History,"²³ read to the annual meeting in 1891, Jameson eloquently pleaded that the United States at least should match what many small European states were already doing in the publication of records. But Congress turned a deaf ear to an AHA memorial in 1894–95, and the association, in the latter year, established its own Historical Manuscripts Commission, headed by Jameson.²⁴

The Historical Manuscripts Commission existed as a standing committee of the AHA through 1935, but, to all intents and purposes, it had ceased to function after 1929. Meanwhile, however, it had published in the annual reports not only numerous guides and inventories to manuscripts in public archives and private hands but also many edited collections of letters, diaries, journals, and the like. The publication of the *National Union Catalog of Manuscript Collections* by the Library of

²¹ The SAA was a daughter organization of the AHA. On December 2, 1934, the AHA's Executive Council appointed a special committee consisting of Albert Ray Newsome, then head of the North Carolina Historical Commission, Francis S. Philbrick of the Law School of the University of Pennsylvania, and Robert C. Binkley of Western Reserve University to report on the relationship of the AHA to "the whole problem of documentary publication and of national, state, local and private archives." The first recommendation in the committee's report (signed only by Newsome and Philbrick), dated October 15, 1935, was the creation of a self-governing organization of professional archivists, which should enjoy the strong support of the AHA. *Annual Report, 1935*, 1 (Washington, 1936): 175–77. Newsome was the first president of the SAA. Another active member of the AHA, Julian Parks Boyd, then librarian of the Historical Society of Pennsylvania and a future president of the AHA (1964), was the first treasurer of the SAA.

²² *AHA Papers*, 1: 64. The Library of Congress, under the leadership of Herbert Putnam, soon answered this call. Putnam brought Worthington C. Ford to the library in 1902 to head the recently established Manuscript Division, and Ford soon set under way a program to copy and photograph large quantities of manuscripts in foreign archives pertaining to early American history. Higham *et al.*, *History: Humanistic Scholarship in America*, 28–29.

²³ *Annual Report, 1891*, 33–61.

²⁴ *Annual Report, 1895* (Washington, 1896), 10. Jameson and a special subcommittee of the AHA tried again, in 1908–09, to secure governmental support for the publication of American historical records. Although President Theodore Roosevelt and Secretary of State Elihu Root supported the proposal, Congress refused to appropriate the money for a national historical commission. Jameson made two other efforts and failed. He then abandoned the project. See Elizabeth Donnan and Leo F. Stock, eds., *An Historian's World: Selections from the Correspondence of John Franklin Jameson* (Philadelphia, 1956), 12–13.

Congress and two private publishers, beginning in 1962 and continuing until the present, in twenty-three large volumes, including four separate index volumes, has made further activity by the AHA in this field unnecessary.

Professional historians in the AHA during its early years were particularly distressed by the almost utter neglect by the government of the United States of its own records. Those records, if kept at all, were, with a few exceptions, arranged and administered by amateurs, were scattered among various departments of the government, and were often stored in buildings that were not fireproof. Jameson issued the first strong call for the establishment of a central national archives in 1890. "Except Switzerland, whose case is peculiar," Jameson said, "I have found no instance of a civilized European country, not even Bavaria, Württemberg, or Baden, which does not spend more absolutely upon its archives than we do."²⁵

If any single institution can be called a monument to one person, then the National Archives is surely a monument to John Franklin Jameson. To be sure, the AHA Executive Council, in 1901, sent a memorial to the two houses of Congress that urged the building of a "hall of records."²⁶ More important, in 1908 the Executive Council appointed a committee to promote the construction of a national archives building to be staffed by trained archivists and scholars. The association petitioned Congress again to this effect in 1910.²⁷ But it was Jameson who kept the pressure on the councils of the AHA and on Congresses and presidents of the United States all through the 1910s and 1920s, until success came with a congressional appropriation in 1926. Ground was broken in 1931, and the present National Archives building was opened for use in 1935.²⁸

The act of 1934 that created the National Archives also established the National Archives Council on which neither the AHA nor any other professional organization was represented. The Executive Council of the AHA, however, did in effect dictate in 1934 the choice of the first Archivist of the United States—Robert Digges Wimberly Connor, the first secretary of the North Carolina Historical Commission and a professor of history at the University of North Carolina. Another provision of the National Archives Act called for creation of the National Historical Publications Commission, two members of which were to be appointed by the AHA. Although the commission was appointed, Congress refused to appropriate any funds for its work, "and the commission lapsed into inactivity."²⁹ There is no time to review the splendid work of the NHPC, which was revived by President Truman in 1950, was later given substantial appropriations, and was enlarged and renamed the National Historical Publications and Records Commission in 1974.³⁰ We can only note here

²⁵ *Annual Report, 1891*, 43.

²⁶ *Annual Report, 1901*, 1 (Washington, 1902): 36.

²⁷ H. G. Jones, *The Records of a Nation* (New York, 1969), 7–8. Also see Donald R. McCoy, *The National Archives: America's Ministry of Documents, 1934–1968* (Chapel Hill, N.C., 1978).

²⁸ Jones, *Records of a Nation*, 8–10; Victor Gondos, Jr., *J. Franklin Jameson and the Birth of the National Archives, 1906–1926* (Philadelphia, 1981); Fred Shelley, "The Interest of J. Franklin Jameson in the National Archives, 1908–1934," *American Archivist*, 22 (1949): 99–130; and Donnan and Stock, *An Historian's World*, 15–17.

²⁹ Jones, *Records of a Nation*, 118–19.

³⁰ For a detailed account of the work of the NHPC to 1968, see *ibid.*, 119–33. For the most recent comprehensive review, see National Historical Publications and Records Commission, *Annual Report for 1982* (Washington, 1984).

that the existence and prodigious work of this agency in promoting and subsidizing the publication of many basic records of American history represented the fruition of the hopes and dreams of Jameson and other founders of the American Historical Association.

This brings us back to the tenuous but usually friendly relationship between the United States government and the American Historical Association. The original act of incorporation of 1889, which has since been amended only slightly, is a very simple document that names certain persons and their successors as "a body corporate and politic, by the name of the American Historical Association, for the promotion of historical studies, the collection and preservation of manuscripts, and for kindred purposes in the interest of American history and of history in America." The association was to have its principal office in Washington and report annually to the secretary of the Smithsonian Institution, who in turn would communicate such reports to Congress as he saw fit. As I have said, publication of the annual reports was a boon to the AHA and a major cause of its growth. But aside from this indirect subsidy, the AHA to my knowledge has never received a dollar appropriated directly by Congress. Nor has it ever applied for or wanted any such appropriations.

The AHA has preferred to act only in an advisory and consultative capacity with various governmental agencies, and even here the initiative for such a relationship has come from these agencies. In recent decades the AHA, in addition to its representation on the NHPRC, has had representatives on advisory councils to the National Archives and Records Service and the Office of the Historian of the State Department. Their work, much of it of a watchdog and supportive nature, has recently been well reviewed³¹ and only needs to be noted and applauded here.

THE PROFESSIONALIZATION OF THE DISCIPLINE of history occurred at the same time that secondary schools were beginning to proliferate in the United States, and the American Historical Association played a determinative role in securing an important place for history in high school curricula and in the training of history teachers in secondary schools. The basic work³² was done by a Committee of Ten, appointed by the National Education Association in 1892. This committee—the first of four influential committees designated by their membership number—was dominated by members of the AHA.³³ In 1896 the association itself appointed the Committee of Seven "to consider the subject of history in the secondary schools and

³¹ Richard W. Leopold, "The Historian and the Federal Government," *Journal of American History*, 64 (1977–78): 5–23.

³² *Report of the Committee on Secondary School Studies Appointed at the Meeting of the National Education Association, July 9, 1892* (Washington, 1893).

³³ The historian members were, notably, Woodrow Wilson of Princeton University, Edward G. Bourne of Adelbert College, Albert Bushnell Hart of Harvard University, and James Harvey Robinson of the University of Pennsylvania. Charles Homer Haskins and Frederick Jackson Turner, both at the University of Wisconsin, also participated by invitation. For an editorial note on the conference and the minutes of its meetings, see Link *et al.*, *Papers of Woodrow Wilson*, 8: 61–73. Also see Theodore R.Sizer, *Secondary Schools at the Turn of the Century* (New Haven, Conn., 1964); and Hazel Whitman Hertzberg, "The Teaching of History," in Kammen, *The Past Before Us*, 475.

to draw up a scheme of college entrance requirements.”³⁴ In 1899 this committee, whose chairperson was Andrew C. McLaughlin of the University of Michigan, recommended a four-year historical curriculum for high schools, consisting of ancient history to A.D. 800, medieval and modern European history, English history, and American history and government, or variations of this program.³⁵

A Committee of Five, appointed by the Executive Council of the AHA in 1907 at the request of the Headmasters' Association, with McLaughlin as chairperson, reported in 1911 that historical courses and teaching methods had been instituted and changed “from one side of the continent to the other” in response to the report of the Committee of Seven. There had, however, recently been much demand for more emphasis on modern history. The Committee of Five, saying that “the desire of teachers to emphasize modern history” had a strong appeal to its members, proposed a four-year program of historical study in high schools that included a full-year course in modern European history and English history since 1760.³⁶ Meanwhile, the Committee of Eight of the AHA, headed by James Alton James of Northwestern University, had produced a report that recommended a program of historical courses in grammar schools.³⁷

Minor curricular changes were undertaken in response to new developments in historiography and the rise of the social sciences. In 1916 a Committee on the Social Studies of the NEA and in 1934 a Commission on the Social Studies of the AHA called for greater room in secondary school curricula for the social sciences.³⁸ The AHA commission, however, was vague as to what the social sciences were and what high school teachers should do with them. Meanwhile, the AHA, alone among the organizations that professed to represent the social sciences, had maintained a close relationship with the National Council for the Social Studies, formed in 1921.³⁹ Whether the nonrecommendations of the AHA's Commission on the Social Studies had any effect is extremely doubtful,⁴⁰ because traditional history continued to enjoy a central place in high school curricula until the 1960s. In any event, the commission was the last AHA committee to address itself in an extensive way to the role history should play in the education of students in secondary schools.

³⁴ Committee of Seven, *The Study of History in Schools: Report to the American Historical Association* (New York, 1899), v.

³⁵ *Ibid.*, 134–36. The other members of the committee were Herbert B. Adams, George L. Fox, rector of the Hopkins Grammar School of New Haven, Albert Bushnell Hart, Charles Homer Haskins of the University of Wisconsin, Lucy M. Salmon of Vassar College, and H. Morse Stephens of Cornell University. It should be noted that, among this group, only Fox was connected with a secondary school. It should also be noted that the committee, in its report of 267 pages, emphasized the central role that history should occupy in high school curricula, discussed “the present condition of history in American secondary schools,” surveyed the place of history in secondary schools in German, French, English, and Canadian schools, discussed the proper training of teachers in history in secondary schools, and so on.

³⁶ Committee of Five, *The Study of History in Secondary Schools: Report to the American Historical Association* (New York, 1911), 64–65.

³⁷ American Historical Association, *The Study of History in Elementary Schools: Report to the American Historical Association by a Committee of Eight* (New York, 1909).

³⁸ [National Education Association], *The Social Studies in Secondary Education* (Washington, 1916); and American Historical Association, *Report of the Commission on the Social Studies: Conclusions and Recommendations* (New York, 1934).

³⁹ Hertzberg, “Teaching of History,” 478–79.

⁴⁰ “The work of the Commission had little or no effect on the teaching in the high schools.” W. Stull Holt, for the AHA Service Center for Teachers of History, *The Historical Profession in the United States* (Washington, 1963), 14.

The AHA used other means to maintain close contact with history teachers in secondary schools. The Council collaborated with a private publisher in 1912 to revive the moribund *History Teacher's Magazine*, which said, in its first issue after its rebirth, that it was "edited under the supervision of a committee of the American Historical Association." The name of the magazine was changed to *Historical Outlook* in October 1918, when it was advertised as "an organ" of the AHA. The National Council for the Social Studies became a cosponsor in 1923. *Historical Outlook* became the *Social Studies* in 1934; three years later, control and direction reverted to the publisher, and all connection between the magazine and the AHA ceased.

In the same month that the *Social Studies* reverted entirely to private control, the AHA and the NCSS launched a new journal for teachers—*Social Education*, "published for the American Historical Association and the National Council for the Social Sciences" by the American Book Company. The NCSS took over publication of *Social Education* in 1941 but still collaborated with the AHA in editorial matters through a joint executive board. In 1955, *Social Education* became the "official journal" of the NCSS, "in collaboration" with the AHA. Since January 1969, *Social Education*, still the official journal of the NCSS, has not mentioned the American Historical Association on its masthead.

While professional historians and the AHA practiced a policy of benign neglect toward secondary schools in the early 1960s, the social scientists—anthropologists, economists, and sociologists—moved into the vacuum and began to reorganize high school curricula. As Hazel Whitman Hertzberg has observed, the social scientists were essentially ahistorical, and the so-called reformers tolerated history only insofar as it could be justified as a social science. Then came the upheavals of the late 1960s and early 1970s. The emphasis was now on "relevance" and "self-realization," "inquiry" and "discovery." As Hertzberg has said: "The new emphasis was both ahistorical and antihistorical. The past was relevant only when it dealt with matters of burning social or personal concern."⁴¹ Richard S. Kirkendall, speaking in 1975 for a committee of the Organization of American Historians, said, "History is in crisis."⁴² It still is, although there are now signs that thoughtful people throughout the country deeply regret the assault on history and the other liberal arts in the secondary schools.

For this turn in opinion, we have to give most of the credit to other publicists and can claim little credit for ourselves.⁴³ To be sure, the AHA's Service Center for Teachers of History did sponsor, between 1958 and 1965, the writing and publication of seventy-four pamphlets on various aspects and fields of history. This excellent series was (and is) a great success, but in all candor it has to be said that most of the essays were written for graduate students and college teachers, not secondary school teachers. And the Teaching Division of the Council has, among

⁴¹ Hertzberg, "Teaching of History," 483.

⁴² "The Status of History in the Schools," *Journal of American History*, 62 (1975–76): 557–70. The quotation is on page 557.

⁴³ For example, see Ernest L. Boyer, *High School: A Report on Secondary Education in America* (New York, 1983); National Commission on Excellence in Education, *A Nation at Risk: The Imperative Need for Educational Reform* (Washington, 1983); Theodore R.Sizer, *The Dilemma of the American High School* (Boston, 1984); and Chester E. Finn, Jr., et al., eds., *The Humanities in America's High Schools* (New York, 1984).

other things, from time to time sponsored regional conferences on the teaching of history in which high school teachers have participated.⁴⁴ But the reports of the vice-presidents of the Teaching Division reveal a constant sense of frustration, a feeling almost of despair, because members of that division have wanted to do many things for which there has been no money.⁴⁵

THESE, THEN, HAVE BEEN SOME OF THE ACHIEVEMENTS and failures of the American Historical Association during its first hundred years. And what of the future, now that we begin the second century of our life? I am quite sure that the American Historical Association will be celebrating its bicentennial one hundred years from now, but, since I am not endowed with the gift of prophecy, I will confine my observations about the future to some activities that I think might well engage our energies in the years immediately ahead.

To begin with, we should always remember that, if we do not stand firmly behind the cause of history and the unfettered freedom of historical inquiry, no one else will do so. A good case in point is what happened to the National Archives when it lost its independence by being under the control of the Federal Records Administration within the new General Services Administration, founded to be the housekeeping agency of the government. The AHA and the Society of American Archivists were utterly indifferent, and the deed was done in the Federal Property and Administrative Services Act of 1949.⁴⁶ A Joint Committee on the Status of the National Archives, with representatives from the AHA, OAH, and SAA, strongly urged in 1968 that the independence of the National Archives be restored.⁴⁷ But the organizations whose deepest interests were at stake did not follow through, and nothing happened.

It has been a far different story since the founding, at the initiative of the AHA, in 1975–76 of the National Coordinating Committee for the Promotion of History. The NCC, under the energetic directorship of Page Putnam Miller, now represents thirty-four historical associations. It has defended our interests on a number of fronts—for example, in protecting the Freedom of Information Act, in leading the fight against the National Security Agency Directive Number 84, and in protecting the funding of the NHPRC and the National Endowment for the Humanities.

To Dr. Miller and the NCC, moreover, must go much of the credit for the achievement of one of the centennial goals of the AHA—the passage of S.905, which reestablished the independence of the National Archives. Many historians and archivists worked hard during 1982–84 for the success of this measure, and we had strong bipartisan support in both houses of Congress. We are particularly grateful to Senators Mark O. Hatfield, Thomas F. Eagleton, and Charles McC.

⁴⁴ Donald B. Cole and Thomas Pressly, *Preparation of Secondary-School History Teachers* (3d edn. rev., Washington, 1983), 27.

⁴⁵ See, particularly, Warren I. Susman's last report as vice-president of the Teaching Division, in *Annual Report, 1978* (Washington, n.d.), 51–59.

⁴⁶ Jones, *Records of a Nation*, 40–65.

⁴⁷ For the report, with Julian P. Boyd's "dissenting statement," see *ibid.*, 275–94. Boyd dissented, he said, because the committee's original draft had been watered down in its final version.

Mathias, Jr., and Representative Jack B. Brooks for seeing the bill through to adoption and to President Reagan for signing it on October 19, 1984. For this epochal victory we can felicitate ourselves and congratulate in particular Dr. Miller. Passage of S.905 ought also to be a reminder of what we can accomplish when we work together with unity and energy. I am also reminded here that, although support for the NCC has increased substantially during the past two years, it is still too small.

Another thing that should engage our attention is the problem of affiliation. At present, some seventy-nine historical societies, with a total membership of almost one hundred thousand persons, are affiliated with the American Historical Association. What troubles me is that no one seems to know what "affiliation" means, aside from the opportunity given to affiliates to hold regular sessions at our annual meetings, although probably not more than 10 percent of their members are also members of the AHA. Is it not possible for the AHA to say that affiliation carries responsibilities as well as privileges? I have asked the Council to review the question of affiliation and to see what might be done to make it meaningful and mutually beneficial, and the Professional Division now has this matter under study.

A task of far greater moment and urgency is the recovery of a crucial role for the AHA in the determination of the curricula of our secondary schools. Let us return to the field that we so unthinkingly abandoned. Let us speak, both as citizens and as professional historians, in the deep conviction that no person can live a full and rich life without intimate knowledge of his or her past. If we do not know where we came from, we cannot know who we are. And if we do not know who we are, then we flounder in ignorance, not knowing where we are going. But there is no need to preach to the converted on this subject. We have talked enough; let us act. In fact, we have under way several initiatives that the Council hopes and believes will bring the AHA back into the mainstream of the teaching of history in our secondary schools.

First, the Teaching Division is now planning a new pamphlet series to be addressed explicitly to the needs of high school teachers. These pamphlets, which will cover United States and world history, will attempt to provide high school teachers with up-to-date surveys of the periods and subjects covered, will comment on new interpretations, and will point out where emphases should be placed and what the current controversies are about. The pamphlets will also include titles of a few books recommended for further reading and perhaps sample lesson plans.

Second, the Council has voted unanimously to authorize the president to appoint a blue-ribbon commission—composed of historians, leaders in secondary school teaching, administration, and governance, and others—to survey, as did the Committee of Seven, the current situation regarding history in our high schools and proper standards for the training of high school history teachers. Numerous states are at this very moment in the process of trying to restore history as an integral part of what is called a basic core discipline. And the leaders in these same states are crying out for help from professional historians. The report of the blue-ribbon commission will come at a propitious time, perhaps at a turning point in the history of American secondary education. But we mean above all to follow through

in concerted action in every state. To this end, we will have the support of the Teaching Division of the AHA, the NCC, and the Committee on Schools and Colleges of the OAH.

I had hoped to announce the appointment of this commission tonight and am disappointed that I cannot do so. But no matter. Plans are now well under way to raise the money necessary for the work of the commission and its staff. And we are determined to emulate our forebears in seeing to it that our children and grandchildren do not grow up in ignorance of their civilization's and their country's past.

These are some of the problems and tasks that lie immediately before us. Meanwhile, as we face the next hundred years, let us, with the same vigor and dedication of our founders, resolve, with strong and active faith in our high calling and acknowledgment of our solemn responsibility, to continue to promote the study of history in the United States.

“Gouge and Bite, Pull Hair and Scratch”: The Social Significance of Fighting in the Southern Backcountry

ELLIOTT J. GORN

“I WOULD ADVISE YOU when You do fight Not to act like Tygers and Bears as these Virginians do—Biting one anothers Lips and Noses off, and *gouging* one another—that is, thrusting out one anothers Eyes, and kicking one another on the Cods, to the Great damage of many a Poor Woman.”¹ Thus, Charles Woodmason, an itinerant Anglican minister born of English gentry stock, described the brutal form of combat he found in the Virginia backcountry shortly before the American Revolution. Although historians are more likely to study people thinking, governing, worshipping, or working, how men fight—who participates, who observes, which rules are followed, what is at stake, what tactics are allowed—reveals much about past cultures and societies.

The evolution of southern backwoods brawling from the late eighteenth century through the antebellum era can be reconstructed from oral traditions and travelers’ accounts. As in most cultural history, broad patterns and uneven trends rather than specific dates mark the way. The sources are often problematic and must be used with care; some speculation is required. But the lives of common people cannot be ignored merely because they leave few records. “To feel for a feller’s eyestrings and make him tell the news” was not just mayhem but an act freighted with significance for both social and cultural history.²

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¹ Woodmason, “Burlesque Sermon,” in Richard J. Hooker, ed., *The Carolina Backcountry on the Eve of the Revolution* (Chapel Hill, 1953), xi–xxxvi, 158. The “Burlesque Sermon” was written in the late 1760s or early 1770s. For the quotation that appears in the title of the essay, see “A Kentucky Fight,” *New York Spirit of the Times*, December 12, 1835, p. 2.

² Harden E. Taliaferro, *Fisher’s River Scenes and Characters* (New York, 1839), 198. Let me state explicitly that this is a study in male culture, but it is informed by central insights of recent women’s history—that gender definitions are malleable, that they have a formative impact on the past, and that to ignore them is to misrepresent social and cultural development.

AS EARLY AS 1735, BOXING was "much in fashion" in parts of Chesapeake Bay, and forty years later a visitor from the North declared that, along with dancing, fiddling, small swords, and card playing, it was an essential skill for all young Virginia gentlemen.³ The term "boxing," however, did not necessarily refer to the comparatively tame style of bare-knuckle fighting familiar to eighteenth-century Englishmen. In 1746, four deaths prompted the governor of North Carolina to ask for legislation against "the barbarous and inhuman manner of boxing which so much prevails among the lower sort of people." The colonial assembly responded by making it a felony "to cut out the Tongue or pull out the eyes of the King's Liege People." Five years later the assembly added slitting, biting, and cutting off noses to the list of offenses. Virginia passed similar legislation in 1748 and revised these statutes in 1772 explicitly to discourage men from "gouging, plucking, or putting out an eye, biting or kicking or stomping upon" quiet peaceable citizens. By 1786 South Carolina had made premeditated mayhem a capital offense, defining the crime as severing another's bodily parts.⁴

Laws notwithstanding, the carnage continued. Philip Vickers Fithian, a New Jerseyite serving as tutor for an aristocratic Virginia family, confided to his journal on September 3, 1774:

By appointment is to be fought this Day near Mr. *Lanes* two fist Battles between four young Fellows. The Cause of the battles I have not yet known; I suppose either that they are lovers, and one has in Jest or reality some way supplanted the other; or has in a merry hour called him a *Lubber* or a *thick-Skull*, or a *Buckskin*, or a *Scotsman*, or perhaps one has mislaid the other's hat, or knocked a peach out of his Hand, or offered him a dram without wiping the mouth of the Bottle; all these, and ten thousand more quite as trifling and ridiculous are thought and accepted as just Causes of immediate Quarrels, in which every diabolical Strategem for Mastery is allowed and practiced.⁵

The "trifling and ridiculous" reasons for these fights had an unreal quality for the matter-of-fact Yankee. Not assaults on persons or property but slights, insults, and thoughtless gestures set young southerners against each other. To call a man a "buckskin," for example, was to accuse him of the poverty associated with leather clothing, while the epithet "Scotsman" tied him to the low-caste Scots-Irish who settled the southern highlands. Fithian could not understand how such trivial offenses caused the bloody battles. But his incomprehension turned to rage when he realized that spectators attended these "odious and filthy amusements" and that the fighters allayed their spontaneous passions in order to fix convenient dates and places, which allowed time for rumors to spread and crowds to gather. The Yankee concluded that only devils, prostitutes, or monkeys could sire creatures so unfit for human society.⁶

³ William Gooch to the Bishop of London, July 8, 1735, in G. McLaren Bryden, ed., "The Virginia Clergy: Governor Gooch's Letters to the Bishop of London, 1727-1749, from the Fulham Manuscripts," *Virginia Magazine of History and Biography*, 32 (1924): 219, 332; and Philip Vickers Fithian to John Peck, August 12, 1774, in Fithian, *Journal and Letters*, ed. Hunter Dickinson Farish (Williamsburg, Va., 1943), 212.

⁴ Tom Parramore, "Gouging in Early North Carolina," *North Carolina Folklore Journal*, 22 (1974): 58; Jane Carson, *Colonial Virginians at Play* (Williamsburg, Va., 1965), 166-67; and Jack Kenny Williams, *Vogues in Villainy: Crime and Retribution in Ante-Bellum South Carolina* (Columbia, S.C., 1959), 33. The South Carolina law included fingers and eyes but excluded noses and ears.

⁵ Fithian, *Journal and Letters*, 240-41.

⁶ *Ibid.*; and Rhys Isaac, *The Transformation of Virginia, 1740-1790* (Chapel Hill, 1982), 44.

Descriptions of these "fist battles," as Fithian called them, indicate that they generally began like English prize fights. Two men, surrounded by onlookers, parried blows until one was knocked or thrown down. But there the similarity ceased. Whereas "Broughton's Rules" of the English ring specified that a round ended when either antagonist fell, southern bruisers only began fighting at this point. Enclosed not inside a formal ring—the "magic circle" defining a special place with its own norms of conduct—but within whatever space the spectators left vacant, fighters battled each other until one called enough or was unable to continue. Combatants boasted, howled, and cursed. As words gave way to action, they tripped and threw, gouged and butted, scratched and choked each other. "But what is worse than all," Isaac Weld observed, "these wretches in their combat endeavor to their utmost to tear out each other's testicles."⁷

Around the beginning of the nineteenth century, men sought original labels for their brutal style of fighting. "Rough-and-tumble" or simply "gouging" gradually replaced "boxing" as the name for these contests.⁸ Before two bruisers attacked each other, spectators might demand whether they proposed to fight fair—according to Broughton's Rules—or rough-and-tumble. Honor dictated that all techniques be permitted. Except for a ban on weapons, most men chose to fight "no holds barred," doing what they wished to each other without interference, until one gave up or was incapacitated.⁹

The emphasis on maximum disfigurement, on severing bodily parts, made this fighting style unique. Amid the general mayhem, however, gouging out an opponent's eye became the sine qua non of rough-and-tumble fighting, much like the knockout punch in modern boxing. The best gougers, of course, were adept at other fighting skills. Some allegedly filed their teeth to bite off an enemy's appendages more efficiently. Still, liberating an eyeball quickly became a fighter's surest route to victory and his most prestigious accomplishment. To this end, celebrated heroes fired their fingernails hard, honed them sharp, and oiled them slick. "You have come off badly this time, I doubt?" declared an alarmed passerby on seeing the piteous condition of a renowned fighter. "Have I," says he triumphantly, shewing from his pocket at the same time an eye,

⁷ Weld, *Travels Through the States of North America*, 1 (3d edn., London, 1800): 191. Weld claimed he saw four or five men castrated and confined to their sick beds during his travels in Virginia and Maryland.

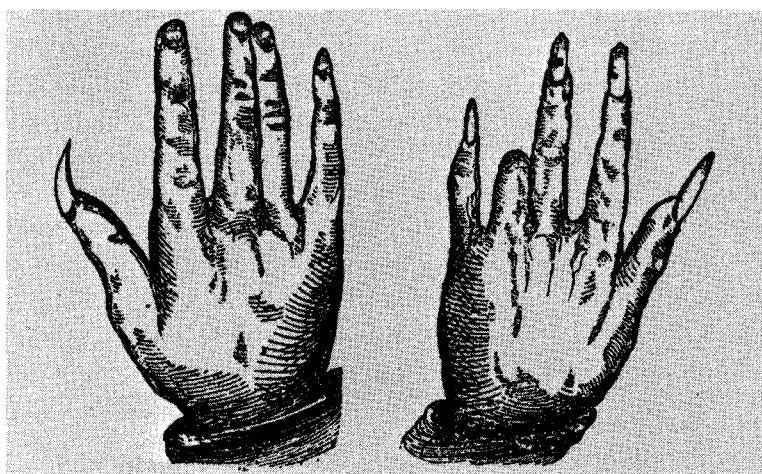
⁸ *The Compact Edition of the Oxford English Dictionary* (New York, 1971), 1: 1180, 2: 2582.

⁹ Thomas Ashe, *Travels in America* (London, 1809), 86. Thomas Anburey, who served in Virginia during the Revolution, observed that fighters agreed ahead of time on which tactics to allow, then abided by their own rules; Anburey, *Travels Through the Interior Parts of America*, 2 (1789; reprint edn., Boston, 1823), 215–18. Gouging another man's eye was not native to the colonies but had antecedents in the mother country. A few reports placed the practice in Lancashire and Yorkshire; the lowland Scots and their descendants in Ulster also used these tactics. Gouging was common enough in English ring fights that the 1838 "Rules of the London Prize Ring" banned it. But what had been an occasional practice in Britain was elevated to a unique fighting style in the American South. See Dr. Beardsley, "On the Use and Abuse of Popular Sports and Exercises, Resembling Those of the Greeks and Romans," *Nicholson's Philosophical Magazine*, 15, excerpted in *Portfolio*, 1, ser. 4 (1816): 407–09; Jennie Holliman, *American Sports, 1785–1835* (Durham, N.C., 1931), chap. 10; *New York Spirit of the Times*, July 4, 1840, p. 207; Henry Adams, *The Formative Years*, ed. Herbert Agar (London, 1948), 28; "'Kick and Bite' in Lancashire," *New York Sporting Magazine*, November 1834, p. 188; John Ford, *Prize Fighting: The Age of Regency Boxmania* (New York, 1971), 116–18; J. C. Furnas, *The Americans: A Social History of the United States, 1587–1914* (New York, 1969), 216; James G. Leyburn, *The Scotch Irish: A Social History* (Chapel Hill, 1962), 263–66; Arthur K. Moore, *The Frontier Mind* (New York, 1957), 111; and Parramore, "Gouging in North Carolina," 56.

which he had extracted during the combat, and preserved for a trophy."¹⁰

As the new style of fighting evolved, its geographical distribution changed. Leadership quickly passed from the southern seaboard to upcountry counties and the western frontier.¹¹ Although examples could be found throughout the South, rough-and-tumbling was best suited to the backwoods, where hunting, herding, and semisubsistence agriculture predominated over market-oriented, staple crop production. Thus, the settlers of western Carolina, Kentucky, and Tennessee, as well as upland Mississippi, Alabama, and Georgia, became especially known for their pugnacity.¹²

The social base of rough-and-tumbling also shifted with the passage of time. Although brawling was always considered a vice of the "lower sort," eighteenth-century Tidewater gentlemen sometimes found themselves in brutal fights. These combats grew out of challenges to men's honor—to their status in patriarchal, kin-



The "Hands of Celebrated Gougers." Drawings reproduced from Richard M. Dorson, *Davy Crockett: American Comic Legend* (New York, 1939), 42.

based, small-scale communities—and were woven into the very fabric of daily life. Rhys Isaac has observed that the Virginia gentry set the tone for a fiercely competitive style of living. Although they valued hierarchy, individual status was never permanently fixed, so men frantically sought to assert their prowess—by grand boasts over tavern gaming tables laden with money, by whipping and

¹⁰ Anburey, *Travels Through the Interior Parts of America*, 203; Parramore, "Gouging in North Carolina," 57–58; and Adland Ashby, *A Visit to North America* (London, 1821), 73. In colonial days, an eye could be saved by calling out "king's curse"; Guion Griggs Johnson, *Antebellum North Carolina: A Social History* (Chapel Hill, 1937), 16–17.

¹¹ The tradition lingered in pockets along the coast. A Florida grand jury member watched outside the courthouse as his son fought another boy. Not yet a decade old, the youngster received some manly advice when the battle ended: "Now you little devil, if you catch him down again bite him, chaw his lip or you never'll be a man." Henry Benjamin Whipple, as quoted in John Hope Franklin, *The Militant South* (Cambridge, Mass., 1956), 11–12.

¹² Tom Parramore, the most thorough student of rough-and-tumble fighting, offered only southern sources and argued that gouging spread as far as the Louisiana Territory early in the century; "Gouging in North Carolina," 56, 58. Gouging was occasionally practiced above the Ohio, but it was not elevated to a characteristic fighting style. Lumbermen in the northern forests practiced some of the rough-and-tumbler's arts, but they were noted for marking a fallen opponent by stomping his face with caulked boots, leaving scars similar to those

tripping each other's horses in violent quarter-races, by wagering one-half year's earnings on the flash of a fighting cock's gaff. Great planters and small shared an ethos that extolled courage bordering on foolhardiness and cherished magnificent, if irrational, displays of largess.¹³

Piety, hard work, and steady habits had their adherents, but in this society aggressive self-assertion and manly pride were the real marks of status. Even the gentry's vaunted hospitality demonstrated a family's community standing, so conviviality itself became a vehicle for rivalry and emulation. Rich and poor might revel together during "public times," but gentry patronage of sports and festivities kept the focus of power clear. Above all, brutal recreations toughened men for a violent social life in which the exploitation of labor, the specter of poverty, and a fierce struggle for status were daily realities.¹⁴

During the final decades of the eighteenth century, however, individuals like Fithian's young gentlemen became less inclined to engage in rough-and-tumbling. Many in the planter class now wanted to distinguish themselves from social inferiors more by genteel manners, gracious living, and paternal prestige than by patriarchal prowess. They sought alternatives to brawling and found them by imitating the English aristocracy. A few gentlemen took boxing lessons from professors of pugilism or attended sparring exhibitions given by touring exponents of the manly art.¹⁵ More important, dueling gradually replaced hand-to-hand combat. The code of honor offered a genteel, though deadly, way to settle personal disputes while demonstrating one's elevated status. Ceremony distinguished anti-septic duels from lower-class brawls. Cool restraint and customary decorum proved a man's ability to shed blood while remaining emotionally detached, to act as mercilessly as the poor whites but to do so with chilling gentility.¹⁶

produced by smallpox. "The lumberjack code," as folklorist Richard Dorson called it, grew out of a pattern of living similar to that of the rough-and-tumblers. Drinking, treating friends, impulsive pleasure seeking, heroic labor, and vicious fighting were part of all-male peer groups in the northern woods; personal honor and valor were the touchstones of lumberjack life. See Dorson, *Bloodstoppers and Bearwalkers: Folk Traditions of the Upper Peninsula* (Cambridge, Mass., 1952), chap. 9; Furnas, *The Americans*, 215-16; and Alan Lomax, *Folksongs of North America* (New York, 1975), 106-07, 119-20. Fred Harvey Harrington has pointed out in private correspondence that leaders of New York City gangs in the mid-nineteenth century were sometimes referred to as gougers or rough-and-tumblers. Moreover, in 1821, Ohio passed a law against gouging out eyes, biting off facial parts, and so forth. Nevertheless, men in the East and Middle West did not glorify mayhem and mutilation in practice and folklore to the same extent as did the southern backwoodsmen. See Gabriel Furman, "The Customs, Amusements, Style of Living and Manners of the People of the United States from the First Settlement to the Present Time," New York Historical Society, New York, N.Y., MS. 2673, typescript copy, pp. 303-05; and Elliott J. Gorn, "The Manly Art: Bare-Knuckle Prize Fighting and the Rise of American Sports" (Ph.D. dissertation, Yale University, 1983), chap. 5.

¹³ Isaac brilliantly evoked life in mid-eighteenth-century Virginia. See *Transformation of Virginia*, chaps. 5, 6. On play, competitiveness, and prowess in southern culture, see T. H. Breen, "Horses and Gentlemen: The Cultural Significance of Gambling among the Gentry of Virginia," *William and Mary Quarterly*, 3d ser., 34 (1977): 256-57; Carson, *Colonial Virginians at Play*, chap. 3; Holliman, *American Sports*, chap. 12; C. Vann Woodward, "The Southern Ethic in a Puritan World," in his *American Counterpoint* (Boston, 1971), 13-46; and Bertram Wyatt-Brown, *Southern Honor: Ethics and Behavior in the Old South* (New York, 1982).

¹⁴ On these themes, see Breen, "Horses and Gentlemen," 256-57; Isaac, *Transformation of Virginia*, 94-104; and Wyatt-Brown, *Southern Honor*, chaps. 2, 3, 6, 11, 13.

¹⁵ Isaac traced this change; *Transformation of Virginia*, pts. 2, 3. Also see Louise Jordan Walmsley, *Sport Attitudes and Practices of Representative Americans Before 1870* (Farmville, Va., 1938), 26; and Gorn, "The Manly Art," 141-54.

¹⁶ Isaac, *Transformation of Virginia*, 319, 322. Also see Dickson Bruce, *Violence and Culture in the Antebellum South* (Austin, 1979), introduction and chap. 1; Wyatt-Brown, *Southern Honor*, chap. 13; and Johnson, *Antebellum North Carolina*, 42-46.

Slowly, then, rough-and-tumble fighting found specific locus in both human and geographical landscapes. We can watch men grapple with the transition. When an attempt at a formal duel aborted, Savannah politician Robert Watkins and United States Senator James Jackson resorted to gouging. Jackson bit Watson's finger to save his eye.¹⁷ Similarly, when "a low fellow who pretends to gentility" insulted a distinguished doctor, the gentleman responded with a proper challenge. "He had scarcely uttered these words, before the other flew at him, and in an instant turned his eye out of the socket, and while it hung upon his cheek, the fellow was barbarous enough to endeavor to pluck it entirely out."¹⁸ By the new century, such ambiguity had lessened, as rough-and-tumble fighting was relegated to individuals in backwoods settlements. For the next several decades, eye-gouging matches were focal events in the culture of lower-class males who still relished the wild ways of old.

"I SAW MORE THAN ONE MAN WHO WANTED AN EYE, and ascertained that I was now in the region of 'gouging,'" reported young Timothy Flint, a Harvard educated, Presbyterian minister bound for Louisiana missionary work in 1816. His spirits buckled as his party turned down the Mississippi from the Ohio Valley. Enterprising farmers gave way to slothful and vulgar folk whom Flint considered barely civilized. Only vicious fighting and disgusting accounts of battles past disturbed their inertia. Residents assured him that the "blackguards" excluded gentlemen from gouging matches. Flint was therefore perplexed when told that a barbarous-looking man was the "best" in one settlement, until he learned that best in this context meant not the most moral, prosperous, or pious but the local champion who had whipped all the rest, the man most dexterous at extracting eyes.¹⁹

Because rough-and-tumble fighting declined in settled areas, some of the most valuable accounts were written by visitors who penetrated the backcountry. Travel literature was quite popular during America's infancy, and many profit-minded authors undoubtedly wrote with their audience's expectations in mind. Images of heroic frontiersmen, of crude but unencumbered natural men, enthralled both writers and readers. Some who toured the new republic in the decades following

¹⁷ William Oliver Stevens, *Pistols at Ten Paces* (Boston, 1940), 33–37; George G. Smith, *The Story of Georgia and the Georgia People, 1732–1860* (Atlanta, 1900), 184; and "Jones' Fight," *New York Spirit of the Times*, January 25, 1840, pp. 559–60, reprinted in *ibid.*, June 15, 1844, p. 181. The author of "Jones' Fight" was anonymous, but clearly the story was derived from oral tradition. Although dueling became a mark of gentlemanly status, social elites sometimes backslid into street brawling during the antebellum period. For examples, see Williams, *Vogues in Villainy*, 23.

¹⁸ Anburey, *Travels Through the Interior Parts of America*, 201–02. Gougers occasionally threatened their social betters. An English traveler in Virginia recalled that his party fled from a small gang—headed by a "veteran cyclops"—that tried to provoke a battle. In Kentucky, years later, Adland Ashby dared not object to the company of one he considered beneath him. To do so, he feared, might cost an eye; *Visit to North America*, 73. Also see the Marquis de Chastellux, *Travels in North America in the Years 1780–1782* (New York, 1828), which was "translated by an English gentleman who resided in America at that period" (translator's note is on pages 292–93).

¹⁹ Flint, *Recollections of the Last Ten Years* (Boston, 1826), 97–98. The right and left banks of the Ohio became a common symbol of the contrast between slave and free states in the writings of foreign travelers. America's most perceptive visitor, Alexis de Tocqueville, included this motif. See *Democracy in America*, ed. Phillips Bradley, 2 vols. (New York, 1945), 1: 376–79.

the Revolution had strong prejudices against America's democratic pretensions. English travelers in particular doubted that the upstart nation—in which the lower class shouted its equality and the upper class was unable or unwilling to exercise proper authority—could survive. Ironically, backcountry fighting became a symbol for both those who inflated and those who punctured America's expansive national ego.

Frontier braggarts enjoyed fulfilling visitors' expectations of backwoods depravity, pumping listeners full of gruesome legends. Their narratives projected a satisfying, if grotesque, image of the American rustic as a fearless, barbaric, larger-than-life democrat. But they also gave Englishmen the satisfaction of seeing their former countrymen run wild in the wilderness. Gouging matches offered a perfect metaphor for the Hobbesian war of all against all, of men tearing each other apart once institutional restraints evaporated, of a heart of darkness beating in the New World. As they made their way from the northern port towns to the southern countryside, or down the Ohio to southwestern waterways, observers concluded that geographical and moral descent went hand in hand. Brutal fights dramatically confirmed their belief that evil lurked in the deep shadows of America's sunny democratic landscape.

And yet, it would be a mistake to dismiss all travelers' accounts of backwoods fighting as fictions born of prejudice. Many sojourners who were sober and careful observers of America left detailed reports of rough-and-tumbles. Aware of the tradition of frontier boasting, they distinguished apocryphal stories from personal observation, wild tales from eye-witness accounts. Although gouging matches became a sort of literary convention, many travelers compiled credible descriptions of backwoods violence.

"The indolence and dissipation of the middling and lower classes of Virginia are such as to give pain to every reflecting mind," one anonymous visitor declared. "Horse-racing, cock-fighting, and boxing-matches are standing amusements, for which they neglect all business; and in the latter of which they conduct themselves with a barbarity worthy of their savage neighbors."²⁰ Thomas Anburey agreed. He believed that the Revolution's leveling of class distinctions left the "lower people" dangerously independent. Although Anburey found poor whites usually hospitable and generous, he was disturbed by their sudden outbursts of impudence, their aversion to labor and love of drink, their vengefulness and savagery. They shared with their betters a taste for gaming, horse racing, and cockfighting, but "boxing matches, in which they display such barbarity, as fully marks their innate ferocious disposition," were all their own. Anburey concluded that an English prize fight was humanity itself compared to Virginia combat.²¹

Another visitor, Charles William Janson, decried the loss of social subordination, which caused the rabble to reinterpret liberty and equality as licentiousness. Paternal authority—the font of social and political order—had broken down in

²⁰ Chastellux, *Travels in North America*, 292–93.

²¹ Anburey, *Travels Through the Interior Parts of America*, 215–18. Also see George W. Featherstonhaugh, *Excursion Through the Slave States from Washington on the Potomac to the Frontier of Mexico*, 2 (London, 1844), 329–30, as cited in Jack K. Williams, *Duelling in the Old South* (College Station, Tex., 1980), 73.

America, as parents gratified their childrens' whims, including youthful tastes for alcohol and tobacco. A national mistrust of authority had brought civilization to its nadir among the poor whites of the South. "The lower classes are the most abject that, perhaps, ever peopled a Christian land. They live in the woods and desarts and many of them cultivate no more land than will raise them corn and cabbages, which, with fish, and occasionally a piece of pickled pork or bacon, are their constant food. . . . Their habitations are more wretched than can be conceived; the huts of the poor of Ireland, or even the meanest Indian wig-wam, displaying more ingenuity and greater industry."²² Despite their degradation—perhaps because of it—Janson found the poor whites extremely jealous of their republican rights and liberties. They considered themselves the equals of their best-educated neighbors and intruded on whomever they chose.²³ The gouging match this fastidious Englishman witnessed in Georgia was the epitome of lower-class depravity:

We found the combatants . . . fast clinched by the hair, and their thumbs endeavoring to force a passage into each other's eyes; while several of the bystanders were betting upon the first eye to be turned out of its socket. For some time the combatants avoided the *thumb stroke* with dexterity. At length they fell to the ground, and in an instant the uppermost sprung up with his antagonist's eye in his hand!!! The savage crowd applauded, while, sick with horror, we galloped away from the infernal scene. The name of the sufferer was John Butler, a Carolinian, who, it seems, had been dared to the combat by a Georgian; and the first eye was for the honor of the state to which they respectively belonged.

Janson concluded that even Indian "savages" and London's rabble would be outraged by the beastly Americans.²⁴

While Janson toured the lower South, his countryman Thomas Ashe explored the territory around Wheeling, Virginia. A passage, dated April 1806, from his *Travels in America* gives us a detailed picture of gouging's social context. Ashe expounded on Wheeling's potential to become a center of trade for the Ohio and upper Mississippi valleys, noting that geography made the town a natural rival of Pittsburgh. Yet Wheeling lagged in "worthy commercial pursuits, and industrious and moral dealings." Ashe attributed this backwardness to the town's frontier ways, which attracted men who specialized in drinking, plundering Indian property, racing horses, and watching cockfights. A Wheeling Quaker assured Ashe that mores were changing, that the underworld element was about to be driven out. Soon, the godly would gain control of the local government, enforce strict observance of the Sabbath, and outlaw vice. Ashe was sympathetic but doubtful. In Wheeling, only heightened violence and debauchery distinguished Sunday from the rest of the week. The citizens' willingness to close up shop and neglect business on the slightest pretext made it a questionable residence for any respectable group of men, let alone a society of Quakers.²⁵

To convey the rough texture of Wheeling life, Ashe described a gouging match.

²² Janson, *The Stranger in America, 1793–1806* (1807; reprint edn., New York, 1935), 304–06, 310–11.

²³ *Ibid.* Edmund S. Morgan argued convincingly that the ideology of white equality was built on the material base of black slavery; Morgan, *American Slavery, American Freedom* (New York, 1975), 376–87, 380–81. Also see George Fredrickson, *The Black Image in the White Mind* (New York, 1971), chaps. 2, 3.

²⁴ Janson, *Stranger in America*, 308–09.

²⁵ Ashe, *Travels in America*, 82–85.

Two men drinking at a public house argued over the merits of their respective horses. Wagers made, they galloped off to the race course. "Two thirds of the population followed: —blacksmiths, shipwrights, all left work: the town appeared a desert. The stores were shut. I asked a proprietor, why the warehouses did not remain open? He told me all good was done for the day: that the people would remain on the ground till night, and many stay till the following morning." Determined to witness an event deemed so important that the entire town went on holiday, Ashe headed for the track. He missed the initial heat but arrived in time to watch the crowd raise the stakes to induce a rematch. Six horses competed, and spectators bet a small fortune, but the results were inconclusive. Umpires' opinions were given and rejected. Heated words, then fists flew. Soon, the melee narrowed to two individuals, a Virginian and a Kentuckian. Because fights were common in such situations, everyone knew the proper procedures, and the combatants quickly decided to "tear and rend" one another—to rough-and-tumble—rather than "fight fair." Ashe elaborated: "You startle at the words tear and rend, and again do not understand me. You have heard these terms, I allow, applied to beasts of prey and to carnivorous animals; and your humanity cannot conceive them applicable to man: It nevertheless is so, and the fact will not permit me the use of any less expressive term."²⁶

The battle began—size and power on the Kentuckian's side, science and craft on the Virginian's. They exchanged cautious throws and blows, when suddenly the Virginian lunged at his opponent with a panther's ferocity. The crowd roared its approval as the fight reached its violent denouement:

The shock received by the Kentuckian, and the want of breath, brought him instantly to the ground. The Virginian never lost his hold; like those bats of the South who never quit the subject on which they fasten till they taste blood, he kept his knees in his enemy's body; fixing his claws in his hair, and his thumbs on his eyes, gave them an instantaneous start from their sockets. The sufferer roared aloud, but uttered no complaint. The citizens again shouted with joy. Doubts were no longer entertained and bets of three to one were offered on the Virginian.

But the fight continued. The Kentuckian grabbed his smaller opponent and held him in a tight bear hug, forcing the Virginian to relinquish his facial grip. Over and over the two rolled, until, getting the Virginian under him, the big man "snapt off his nose so close to his face that no manner of projection remained." The Virginian quickly recovered, seized the Kentuckian's lower lip in his teeth, and ripped it down over his enemy's chin. This was enough: "The Kentuckian at length *gave out*, on which the people carried off the victor, and he preferring a triumph to a doctor, who came to cicatrize his face, suffered himself to be chaired round the ground as the champion of the times, and the first *rougher-and-tumbler*. The poor wretch, whose eyes were started from their spheres, and whose lip refused its office, returned to the town, to hide his impotence, and get his countenance repaired." The citizens refreshed themselves with whiskey and biscuits, then resumed their races.

²⁶ *Ibid.*, 85–86.

Ashe's Quaker friend reported that such spontaneous races occurred two or three times a week and that the annual fall and spring meets lasted fourteen uninterrupted days, "aided by the licentious and profligate of all the neighboring states." As for rough-and-tumbles, the Quaker saw no hope of suppressing them. Few nights passed without such fights; few mornings failed to reveal a new citizen with mutilated features. It was a regional taste, unrestrained by law or authority, an inevitable part of life on the left bank of the Ohio.²⁷

BY THE EARLY NINETEENTH CENTURY, ROUGH-AND-TUMBLE fighting had generated its own folklore.²⁸ Horror mingled with awe when residents of the Ohio Valley pointed out one-eyed individuals to visitors, when New Englanders referred to an empty eye socket as a "Virginia Brand," when North Carolinians related stories of mass rough-and-tumbles ending with eyeballs covering the ground, and when Kentuckians told of battle-royals so intense that severed eyes, ears, and noses filled bushel baskets. Place names like "Fighting Creek" and "Gouge Eye" perpetuated the memory of heroic encounters, and rustic bombast reached new extremes with estimates from some counties that every third man wanted an eye.²⁹ As much as the style of combat, the rich oral folklore of the backcountry—the legends, tales, ritual boasts, and verbal duels, all of them in regional vernacular—made rough-and-tumble fighting unique.

It would be difficult to overemphasize the importance of the spoken word in southern life. Traditional tales, songs, and beliefs—transmitted orally by blacks as well as whites—formed the cornerstone of culture. Folklore socialized children, inculcated values, and helped forge a distinct regional sensibility. Even wealthy and well-educated planters, raised at the knees of black mammies, imbibed both Afro-American and white traditions, and charismatic politicians secured loyal followers by speaking the people's language. Southern society was based more on personalistic, face-to-face, kin-and-community relationships than on legalistic or bureaucratic ones. Interactions between southerners were guided by elaborate rituals of hospitality, demonstrative conviviality, and kinship ties—all of which emphasized personal dependencies and reliance on the spoken word. Through the antebellum period and beyond, the South had an oral as much as a written culture.³⁰

Boundaries between talk and action, ideas and behavior, are less clear in spoken than in written contexts. Psychologically, print seems more distant and abstract than speech, which is inextricably bound to specific individuals, times, and places. In

²⁷ *Ibid.*, 86–88. No doubt Ashe exaggerated the frequency of gouging matches.

²⁸ Walter Blair and Franklin J. Meine, *Mike Fink, King of Mississippi Keelboatmen* (New York, 1933), 105–25.

²⁹ *New York Spirit of the Times*, July 4, 1840, p. 207; Parramore, "Gouging in North Carolina," 62; Moore, *Frontier Mind*, 112; Horace Kephart, *Our Southern Highlands* (New York, 1929), 375; and Weld, *Travels Through the States of North America*, 193.

³⁰ See Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), esp. bk. 1, pt. 1; Frank Lawrence Owsley, *Plain Folk of the Old South* (Baton Rouge, 1949); Bruce, *Violence and Culture*, introduction and chaps. 1–3, 8; Isaac, *Transformation of Virginia*, 121–31; and Steven Hahn, *The Roots of Southern Populism* (New York, 1983), chaps. 1, 2. Wyatt-Brown observed that honor and shame in southern culture reinforced the importance of the spoken word because they required personal confrontations; *Southern Honor*, 46–48, 56–58, and pt. 3.

becoming part of the realm of sight rather than sound, words leave behind their personal, living qualities, gaining in fixity what they lose in dynamism. Literate peoples separate thought from action, pigeon-holing ideas and behavior. Nonliterate ones draw this distinction less sharply, viewing words and the events to which they refer as a single reality. In oral cultures generally, and the Old South in particular, the spoken word was a powerful force in daily life, because ideation and behavior remained closely linked.³¹

The oral traditions of hunters, drifters, herdsman, gamblers, roustabouts, and rural poor who rough-and-tumbled provided a strong social cement. Tall talk around a campfire, in a tavern, in front of a crossroads store, or at countless other meeting places on the southwestern frontier helped establish communal bonds between disparate persons. Because backwoods humorists possessed an unusual ability to draw people together and give expression to shared feelings, they often became the most effective leaders and preachers.³² But words could also divide. Fithian's observation in the eighteenth century—that seemingly innocuous remarks led to sickening violence—remained true for several generations. Men were so touchy about their personal reputations that any slight required an apology. This failing, only retribution restored public stature and self-esteem. "Saving face" was not just a metaphor.³³

The lore of backwoods combat, however, both inflated and deflated egos. By the early nineteenth century, simple epithets evolved into verbal duels—rituals well known to folklorists. Backcountry men took turns bragging about their prowess, possessions, and accomplishments, spurring each other on to new heights of self-magnification.³⁴ Such exchanges heightened tension and engendered a sense of theatricality and display. But boasting, unlike insults, did not always lead to combat, for, in a culture that valued oral skills, the verbal battle itself—the contest over who best controlled the power of words—was a real quest for domination:

"I am a man; I am a horse; I am a team. I can whip any man in all Kentucky, by G-d!" The other replied, "I am an alligator, half man, half horse; can whip any man on the Mississippi, by G-d!" The first one again, "I am a man; have the best horse, best dog, best gun and handsomest wife in all Kentucky, by G-d." The other, "I am a Mississippi snapping turtle: have bear's claws, alligator's teeth, and the devil's tail; can whip any man, by G-d."³⁵

³¹ Lawrence W. Levine, *Black Culture and Black Consciousness* (New York, 1978), 157. Levine's work is indispensable for historians studying southern folk cultures, black or white.

³² Kenneth Schuyler Lynn, *Mark Twain and Southwestern Humor* (Boston, 1960), 23–32.

³³ Harden Taliaferro created a character who incited others to fight by inadvertently uttering Latin phrases, making them feel intellectually inferior; *Fisher's River Scenes*, 193–94. Mike Fink once challenged a man who failed to laugh at his stories, claiming that the stranger's sullenness dampened everyone's spirits; Blair and Meine, *Mike Fink*, 112–13. For the theme of defending reputation, see Peter Berger et al., *The Homeless Mind* (New York, 1973), 83–96; Wyatt-Brown, *Southern Honor*, 14–15; and Edward L. Ayers, *Vengeance and Justice, Crime and Punishment in the Nineteenth-Century American South* (New York, 1984), chap. 1.

³⁴ For example, see C. F. Hoffman, *A Winter in the West, By a New Yorker*, 2 (New York, 1835), 221–24, as cited in James I. Robertson, Jr., "Frolics, Fights, and Firewater in Frontier Tennessee," *Tennessee Historical Quarterly*, 17 (1958): 97. Whites' sensitivity to affront can be contrasted with blacks who made ritualized insult into a dueling game commonly called "the dozens." Playing the dozens toughened blacks for the abuse that whites inevitably gave them. See, for example, Levine, *Black Culture and Black Consciousness*, 344–58; John Dollard, "The Dozens: Dialect of Insult," *American Imago*, 1 (1939): 3–25; and Roger D. Abrahams, "Playing the Dozens," *Journal of American Folklore*, 75 (1962): 209–20. Both articles are reprinted in Alan Dundes, *Mother Wit from the Laughing Barrel* (Englewood Cliffs, N.J., 1973), 277–94, 295–309.

³⁵ Thus, Christian Schultz, Jr., overheard two drunken riverboatmen arguing over a Choctaw woman;

Such elaborate boasts were not composed on the spot. Folklorists point out that free-phrase verbal forms, from Homeric epics to contemporary blues, are created through an oral formulaic process. The singer of epics, for example, does not memorize thousands of lines but knows the underlying skeleton of his narrative and, as he sings, fleshes it out with old commonplaces and new turns of phrase. In this way, oral formulaic composition merges cultural continuity with individual creativity. A similar but simplified version of the same process was at work in backwoods bragging.³⁶

A quarter-century after the above exchange made its way into print, several of the same phrases still circulated orally and were worked into new patterns. "By Gaud, stranger," said he, 'do you know me?—do you know what stuff I'm made of? Clear steamboat, sea horse, alligator—run agin me, run agin a snag—jam up—whoop! Got the prettiest sister, and biggest whiskers of any man hereabouts—I can lick my weight in wild cats, or any man in all Kentuck!'"³⁷ Style and details changed, but the themes remained the same: comparing oneself to wild animals, boasting of possessions and accomplishments, asserting domination over others. Mike Fink, legendary keelboatman, champion gouger, and fearless hunter, put his own mark on the old form and elevated it to art:

"I'm a salt River roarer! I'm a ring tailed squealer! I'm a regular screamer from the old Massassip! Whoop! I'm the very infant that refused his milk before its eyes were open and called out for a bottle of old Rye! I love the women and I'm chockful o' fight! I'm half wild horse and half cock-eyed alligator and the rest o' me is crooked snags an' red-hot snappin' turtle. . . . I can out-run, out-jump, out-shoot, out-brag, out-drink, an' out-fight, rough-an'-tumble, no holts barred, any man on both sides the river from Pittsburgh to New Orleans an' back ag'in to St. Louiee. Come on, you flatters, you bargers, you milk white mechanics, an' see how tough I am to chew! I ain't had a fight for two days an' I'm spilein' for exercise. Cock-a-doodle-doo!"³⁸

Tall talk and ritual boasts were not uniquely American. Folklore indexes are filled with international legends and tales of exaggeration.³⁹ But inflated language did find a secure home in America in the first half of the nineteenth century. Spread-eagle rhetoric was tailor-made for a young nation seeking a secure identity. Bombastic speech helped justify the development of unfamiliar social institutions, flowery oratory salved painful economic changes, and lofty words masked aggressive territorial expansion. In a circular pattern of reinforcement, heroic talk spurred heroic deeds, so that great acts found heightened meaning in great words. Alexis de Tocqueville observed during his travels in the 1830s that clearing land, draining swamps, and planting crops were hardly the stuff of literature. But the

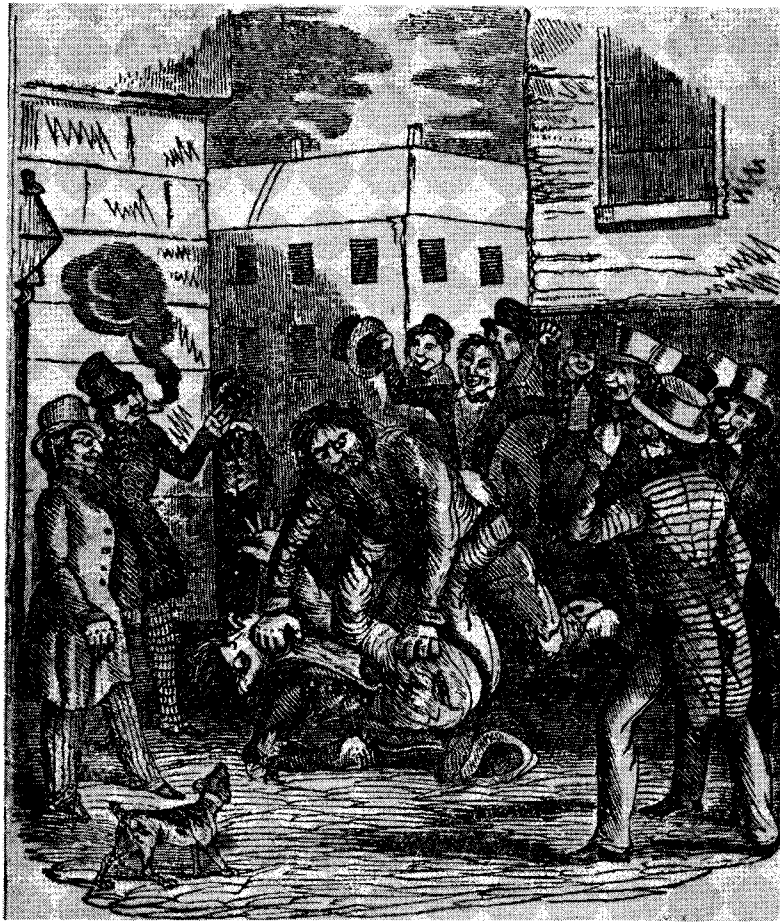
Moore, *Frontier Mind*, 115. Also see Richard M. Dorson, ed., *Davy Crockett, American Comic Legend* (New York, 1939), xv–xvii.

³⁶ Albert B. Lord and Milman Perry found that, despite the passage of decades, Serbo-Croatian epics changed in detail but not in plot or structure. Lord described their field studies in his *The Singer of Tales* (New York, 1971), chaps. 1–3. Also see William R. Ferris, Jr., *Blues from the Delta* (New York, 1978), sect. 2.

³⁷ Lynn, *Mark Twain and Southwestern Humor*, 27.

³⁸ Blair and Meine, *Mike Fink*, 105–06.

³⁹ Stith Thompson, *The Folktale* (New York, 1946), and *Motif Index of Folk Literature* (Bloomington, Ind., 1955–58).



"A Street Fight," *The Crockett Almanac*, 1841 (Nashville, Tenn., [1840]). Woodcut reproduced courtesy of Lilly Library, Indiana University, Bloomington.

collective vision of democratic multitudes building a great nation formed a grand poetic ideal that haunted men's imaginations.⁴⁰

The gaudy poetry of the strapping young nation had its equivalent in the exaggeration of individual powers. Folklore placing man at the center of the universe buttressed the emergent ideology of equality. Tocqueville underestimated Americans' ability to celebrate the mundane, for ego magnification was essential in a nation that extolled self-creation. While America prided itself on shattering old boundaries, on liberating individuals from social, geographic, and cultural encumbrances, such freedom left each citizen frighteningly alone to succeed or fail in forging his own identity. To hyperbolize one's achievements was a source of power and control, a means of amplifying the self while bringing human, natural, and social obstacles down to size. The folklore of exaggeration could transform even the most prosaic commercial dealings into great contests. Early in the nineteenth century, legends of crafty Yankee peddlers and unscrupulous livestock traders abounded.⁴¹ A horse dealer described an animal to a buyer in the 1840s: "Sir, he can jump a house or go through a pantry, as it suits him; no hounds are too fast for

⁴⁰ Tocqueville, *Democracy in America*, 2: 75–83.

⁴¹ See, for example, Eugene W. Hollon, *Frontier Violence, Another Look* (New York, 1974), 21; "A Kentucky

him, no day too long for him. He has the courage of a lion, and the docility of a lamb, and you may ride him with a thread. Weight did you say? Why, he would carry the national debt and not bate a penny." The most insipid marketplace transactions were transfigured by inflated language, legends of heroic salesman-ship, and an ethos of contest and battle.⁴²

The oral narratives of the southern backcountry drew strength from these national traditions yet possessed unique characteristics. Above all, fight legends portrayed backwoodsmen reveling in blood. Violence existed for its own sake, unencumbered by romantic conventions and claiming no redeeming social or psychic value. Gouging narratives may have masked grimness with black humor, but they offered little pretense that violence was a creative or civilizing force.⁴³ Thus, one Kentuckian defeated a bear by chewing off its nose and scratching out its eyes. "They can't stand Kentucky play," the settler proclaimed, "biting and gouging are too hard for them." Humor quickly slipped toward horror, when Davy Crockett, for example, coolly boasted, "I kept my thumb in his eye, and was just going to give it a twist and bring the peeper out, like taking up a gooseberry in a spoon." To Crockett's eternal chagrin, someone interrupted the battle just at this crucial juncture.⁴⁴

Sadistic violence gave many frontier legends a surreal quality. Two Mississippi raftsmen engaged in ritual boasts and insults after one accidentally nudged the other toward the water, wetting his shoes. Cheered on by their respective gangs, they stripped off their shirts, then pummeled, knocked out teeth, and wore skin from each other's faces. The older combatant asked if his opponent had had enough. "Yes," he was told, "when I drink your heart's blood, I'll cry enough, and not till then." The younger man gouged out an eye. Just as quickly, his opponent was on top, strangling his adversary. But in a final reversal of fortunes, the would-be victor cried out, then rolled over dead, a stab wound in his side. Protected by his clique, the winner jumped in the water, swam to a river island, and crowed: "Ruoo-ruoo-o! I can lick a steamboat. My fingernails is related to a sawmill on my mother's side and my daddy was a double breasted catamount! I wear a hoop snake for a neck-handkerchief, and the brass buttons on my coat have all been boiled in poison."⁴⁵

Fight," *New York Spirit of the Times*, December 12, 1835, p. 2; Richard M. Dorson, *America in Legend* (New York, 1973), 57-122; Neil Harris, *Humbug: The Art of P. T. Barnum* (Boston, 1973), 67-89; and Lawrence W. Levine, "William Shakespeare and the American People: A Study in Cultural Transformation," *AHR*, 89 (1984): 53-54.

⁴² *New York Spirit of the Times*, May 30, 1846, p. 159. Legends of frontier fighting found their way into comic almanacs and dime novels and onto the urban stage. Grandiose boasts and legendary fights fed a national tradition celebrating larger-than-life heroes. See Blair and Meine, *Mike Fink*, 105-25; Constance Rourke, *American Humor* (New York, 1931), 53-55; Dorson, *Davy Crockett*, xv-xxvi, 34-35, 38-42, 60-61, 127-30; and Robertson, "Frolics, Fights, and Firewater," 97-99.

⁴³ For the most wide-ranging discussion of violence in American fiction, see Richard Slotkin, *Regeneration Through Violence* (Middletown, Conn., 1973), esp. chaps. 9-13. Also see David Brion Davis, *Homicide in American Fiction, 1798-1860* (Ithaca, 1957); and Kenneth Schuyler Lynn, "Violence in American Literature and Folklore," in Hugh Davis Graham and Ted Robert Gurr, eds., *Violence in America: Historical and Comparative Perspectives* (New York, 1969), 226-45.

⁴⁴ James B. Finley, as quoted in Moore, *Frontier Mind*, 87; and Dorson, *Davy Crockett*, 83. Another Kentuckian fought an alligator and insisted that his comrades stay back and "give the fellow fair play." The alligator, of course, lost both eyes. Moore, *Frontier Mind*, 87.

⁴⁵ "An Arkansas Fight," *New York Spirit of the Times*, February 18, 1843, p. 611. For an analysis of the graphic realism of such stories, see Norris Yates, *William T. Porter and the Spirit of the Times* (Baton Rouge, 1957), 118-22.

The danger and violence of daily life in the backwoods contributed mightily to sanguinary oral traditions that exalted the strong and deprecated the weak. Early in the nineteenth century, the Southwest contained more than its share of terrifying wild animals, powerful and well-organized Indian tribes, and marauding white outlaws. Equally important were high infant mortality rates and short life expectancies, agricultural blights, class inequities, and the centuries-old belief that betrayal and cruelty were man's fate. Emmeline Grangerford's graveyard poetry—set against a backdrop of rural isolation shattered by sadistic clan feuds—is but the best-known expression of the deep loneliness, death longings, and melancholy that permeated backcountry life.⁴⁶

At first glance, boisterous tall talk and violent legends seem far removed from sadness and alienation. Yet, as Kenneth Lynn has argued, they grew from common origins, and the former allowed men to resist succumbing to the latter. Not passive acceptance but identification with brutes and brawlers characterized frontier legendry. Rather than be overwhelmed by violence, acquiesce in an oppressive environment, or submit to death as an escape from tragedy, why not make a virtue of necessity and flaunt one's unconcern? To revel in the lore of deformity, mutilation, and death was to beat the wilderness at its own game.⁴⁷ The storyteller's art dramatized life and converted nameless anxieties into high adventure; bravado helped men face down a threatening world and transform terror into power. To claim that one was sired by wild animals, kin to natural disasters, and tougher than steam engines—which were displacing rivermen in the antebellum era—was to gain a momentary respite from fear, a cathartic, if temporary, sense of being in control. Symbolically, wild boasts overwhelmed the very forces that threatened the backwoodsmen.

But there is another level of meaning here. Sometimes fight legends invited an ambiguous response, mingling the celebration of beastly acts with the rejection of barbarism. By their very nature, tall tales elicit skepticism. Even while men identified with the violence that challenged them, the folklore of eye gouging constantly tested the limits of credibility.⁴⁸ "Pretty soon I got the squatter down, and just then he fixed his teeth into my throte, and I felt my windpipe begin to loosen."⁴⁹ The calculated coolness and understatement of this description highlights the outrageousness of the act. The storyteller has artfully maneuvered his audience to the edge of credulity.

Backwoodsmen mocked their animality by exaggerating it, thereby affirming their own humanity. A Kentuckian battled inconclusively from ten in the morning until sundown, when his wife showed up to cheer him on:

"So I gathered all the little strength I had, and I socked my thumb in his eye, and with my

⁴⁶ For two examinations of the deep pessimism and melancholy pervading backwoods life, see Wyatt-Brown, *Southern Honor*, 29–34; and Bruce, *Violence and Culture*, chap. 4. Also see Mark Twain, *The Adventures of Huckleberry Finn* (1884; reprint, New York, 1959), 104–07.

⁴⁷ Lynn, *Mark Twain and Southwestern Humor*, 23–32.

⁴⁸ Rourke, *American Humor*, chap. 2. Neil Harris argued that stretching the limits of credulity was precisely the appeal of P. T. Barnum. In a democratic society, individuals must distinguish sham from truth, the very game Barnum played with his audience; *Humbug*, 67–89.

⁴⁹ Dorson, *Davy Crockett*, 83.

fingers took a twist on his *snot box*, and with the other hand, I grabbed him by the back of the head; I then caught his ear in my mouth, gin his head a flirt, and *out come his ear by the roots!* I then flopped his head over, and caught his other ear in my mouth, and jerked that out in the same way, and it made a hole in his head that I could have rammed my fist through, and I was just goin' to when he hollered: 'Nuff!'"⁵⁰

More than realism or fantasy alone, fight legends stretched the imagination by blending both. As metaphoric statements, they reconciled contradictory impulses, at once glorifying and parodying barbarity. In this sense, gouging narratives were commentaries on backwoods life. The legends were texts that allowed plain folk to dramatize the tensions and ambiguities of their lives: they hauled society's goods yet lived on its fringe; they destroyed forests and game while clearing the land for settlement; they killed Indians to make way for the white man's culture; they struggled for self-sufficiency only to become ensnared in economic dependency. Fight narratives articulated the fundamental contradiction of frontier life—the abandonment of "civilized" ways that led to the ultimate expansion of civilized society.⁵¹

FOREIGN TRAVELERS MIGHT EXAGGERATE and backwoods storytellers embellish, but the most neglected fact about eye-gouging matches is their actuality.⁵² Circuit Court Judge Aedamus Burke barely contained his astonishment while presiding in South Carolina's upcountry: "Before God, gentlemen of the jury, I never saw such a thing before in the world. There is a plaintiff with an eye out! A juror with an eye out! And two witnesses with an eye out!" If the "ringtailed roarers" did not actually breakfast on stewed Yankee, washed down with spike nails and epsom salts, court records from Sumner County, Arkansas, did describe assault victims with the words "nose was bit." The gamest "gamecock of the wilderness" never really moved steamboat engines by grinning at them, but Reuben Cheek did receive a three-year sentence to the Tennessee penitentiary for gouging out William Maxey's eye.⁵³ Most backcountrymen went to the grave with their faces intact, just as most of the southern gentry never fought a duel. But as an extreme version of the common tendency toward brawling, street fighting, and seeking personal vengeance, rough-

⁵⁰ Moore, *Frontier Mind*, 112.

⁵¹ In *Regeneration Through Violence*, Slotkin argued that this contradiction was the very font of literature and folklore on the American frontier. Also see Lynn, *Mark Twain and Southwestern Humor*, 23–32.

⁵² Folklorists and literary scholars, primarily interested in textual analysis, too readily dismiss the reality of these battles. See, for example, Walter Blair and Hamlin Hill, *America's Humor from Poor Richard to Doonesbury* (New York, 1978), 113–32; and Lynn, *Mark Twain and Southwestern Humor*, 23–32.

⁵³ Benjamin F. Perry, as quoted in Williams, *Vogues in Villainy*, 33; Dorson, *Davy Crockett*, xv–xvi; and Robertson, "Frolics, Fights, and Firewater," 109. Perry's diary, 1832–60, is in the Southern Historical Collection of the University of North Carolina, Chapel Hill. Williams also recounted the case of a judge who sentenced two defendants—one missing his lip, the other an ear—to the same cell: "[Now] you may bite one another as much as you please"; *Vogues in Villainy*, 33. Wyatt-Brown included the following account: "In Davidson County, North Carolina, a drunken young mountaineer named William Tippet had bitten off a large piece of old Arthur Newsome's chin, almost plucked out his left eye, and grasped Newsome's right eye with his other hand. A witness at the tavern scene reported that Tippet 'felt the eyeball slip around his fingers,' and said with a laugh before the crowd watching that 'he reckoned the fire flew mightily' out of that eye. Indeed, the old man was left with just one, badly injured, eye when the right one popped out some days later." *Southern Honor*, 393.

and-tumbling gives us insight into the deep values and assumptions—the *mentalité*—of backwoods life.⁵⁴

Observers often accused rough-and-tumblers of fighting like animals. But eye gouging was not instinctive behavior, the human equivalent of two rams vying for dominance. Animals fight to attain specific objectives, such as food, sexual priority, or territory. Precisely where to draw the line between human aggression as a genetically programmed response or as a product of social and cultural learning remains a hotly debated issue. Nevertheless, it would be difficult to make a case for eye gouging as a genetic imperative, coded behavior to maximize individual or species survival. Although rough-and-tumble fighting appears primitive and anarchic to modern eyes, there can be little doubt that its origins, rituals, techniques, and goals were emphatically conditioned by environment; gouging was learned behavior. Humanistic social science more than sociobiology holds the keys to understanding this phenomenon.⁵⁵

What can we conclude about the culture and society that nourished rough-and-tumble fighting? The best place to begin is with the material base of life and the nature of daily work. Gamblers, hunters, herders, roustabouts, rivermen, and yeomen farmers were the sorts of persons usually associated with gouging. Such hallmarks of modernity as large-scale production, complex division of labor, and regular work rhythms were alien to their lives. Recent studies have stressed the premodern character of the southern uplands through most of the antebellum period. Even while cotton production boomed and trade expanded, a relatively small number of planters owned the best lands and most slaves, so huge parts of the South remained outside the flow of international markets or staple crop agriculture. Thus, backcountry whites commonly found themselves locked into a semisub-sistent pattern of living. Growing crops for home consumption, supplementing food supplies with abundant game, allowing small herds to fatten in the woods, spending scarce money for essential staples, and bartering goods for the services of part-time or itinerant trades people, the upland folk lived in an intensely local, kin-based society. Rural hamlets, impassable roads, and provincial isolation—not growing towns, internal improvements, or international commerce—characterized the backcountry.⁵⁶

⁵⁴ On the remarkably high rate of interpersonal violence in the South, see Ayers, *Vengeance and Justice*, 9–33, 98–101, 111–16, 263–76; Michael S. Hindus, *Prison and Plantation: Crime, Justice, and Authority in Massachusetts and South Carolina, 1767–1878* (Chapel Hill, 1980), 42–49, 63–67, 96–98; and Williams, *Vogues in Villainy*, 6–7, 11–14, 31–38.

⁵⁵ The nature or nurture debate rages on. For examples, see Erich Fromm, *The Anatomy of Human Destructiveness* (New York, 1973), pts. 1, 2; Clifford Geertz, "The Growth of Culture and the Evolution of the Mind," in his *The Interpretation of Cultures* (New York, 1973), chap. 3; Lionel Tiger, *Men in Groups* (New York, 1969); Richard G. Sipes, "War, Sports, and Aggression: An Empirical Test of Two Rival Theories," *American Anthropologist*, new ser., 75 (1973): 64–86; Edward O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, Mass., 1975); Charles J. Lumsden and Edward O. Wilson, *Promethean Fire: Reflections on the Origin of Mind* (Cambridge, Mass., 1982); Daniel G. Freedman, *Human Sociobiology* (New York, 1979); Ashley Montagu, ed., *Sociobiology Examined* (New York, 1980); Michael S. Gregory et al., *Not in Our Genes: Biology, Ideology, and Human Nature* (New York, 1984); Marshall Sahlins, *The Use and Abuse of Biology* (Ann Arbor, 1976); Stephen Jay Gould, "Genes on the Brain," *New York Review*, June 30, 1983, pp. 5–10; and Peter Marsh and Anne Campbell, eds., *Aggression and Violence* (Oxford, 1982).

⁵⁶ Moore, *Frontier Mind*, 114–18; Owsley, *Plain Folk of the Old South*, chap. 3; and Wyatt-Brown, *Southern Honor*, esp. chap. 2. Several recent studies have emphasized the premodern, localistic social and cultural

Even men whose livelihoods depended on expanding markets often continued their rough, premodern ways. Characteristic of life on a Mississippi barge, for example, were long periods of idleness shattered by intense anxiety, as deadly snags, shoals, and storms approached. Running aground on a sandbar meant backbreaking labor to maneuver a thirty-ton vessel out of trouble. Boredom weighed as heavily as danger, so tale telling, singing, drinking, and gambling filled the empty hours. Once goods were taken on in New Orleans, the men began the thousand-mile return journey against the current. Before steam power replaced muscle, bad food and whiskey fueled the gangs who day after day, exposed to wind and water, poled the river bottoms or strained at the cordelling ropes until their vessel reached the tributaries of the Missouri or the Ohio. Hunters, trappers, herdsman, subsistence farmers, and other backwoodsmen faced different but equally taxing hardships, and those who endured prided themselves on their strength and daring, their stamina, cunning, and ferocity.⁵⁷

Such men played as lustily as they worked, counterpointing bouts of intense labor with strenuous leisure. What travelers mistook for laziness was a refusal to work and save with compulsive regularity. "I have seen nothing in human form so profligate as they are," James Flint wrote of the boatmen he met around 1820. "Accomplished in depravity, their habits and education seem to comprehend every vice. They make few pretensions to moral character; and their swearing is excessive and perfectly disgusting. Although earning good wages, they are in the most abject poverty; many of them being without anything like clean or comfortable clothing." A generation later, Mark Twain vividly remembered those who manned the great timber and coal rafts gliding past his boyhood home in Hannibal, Missouri: "Rude, uneducated, brave, suffering terrific hardships with sailorlike stoicism; heavy drinkers, course frolickers in moral sties like the Natchez-under-the-hill of that day, heavy fighters, reckless fellows, every one, elephantinely jolly, foul witted, profane; prodigal of their money, bankrupt at the end of the trip, fond of barbaric finery, prodigious braggarts; yet, in the main, honest, trustworthy, faithful to promises and duty, and often picaresquely magnanimous." Details might change, but penury, loose morality, and lack of steady habits endured.⁵⁸

Boatmen, hunters, and herdsman were often separated from wives and children for long periods. More important, backcountry couples lacked the emotionally intense experience of the bourgeois family. They spent much of their time apart and found companionship with members of their own sex. The frontier town or crossroads tavern brought males together in surrogate brotherhoods, where rough men paid little deference to the civilizing role of women and the moral uplift of the domestic family. On the margins of a booming, modernizing society, they shared an

patterns of the upland South and their transformation during the middle decades of the nineteenth century. See Hahn, *Roots of Populism*, pt. 1; J. Mills Thornton III, *Politics and Power in a Slave Society: Alabama, 1800-1860* (Baton Rouge, 1978), chaps. 1, 5; William L. Barney, *The Secessionist Impulse: Alabama and Mississippi in 1860* (Princeton, 1974), chap. 1; and Forrest McDonald and Grady McWhiney, "The South from Self-Sufficiency to Peonage: An Interpretation," *AHR*, 85 (1980): 1103-11.

⁵⁷ Moore, *Frontier Mind*, 117-21; Bruce, *Violence and Culture*, chaps. 4, 9; Owsley, *Plain Folk of the Old South*, chap. 3; and Malcolm J. Rohrbough, *The Trans-Appalachian Frontier* (New York, 1978), 283-84.

⁵⁸ Flint, as quoted in Moore, *Frontier Mind*, 115; and Twain, *Life on the Mississippi* (1883; reprint, New York, 1961), 24.

intensely communal yet fiercely competitive way of life. Thus, where work was least rationalized and specialized, domesticity weakest, legal institutions primitive, and the market economy feeble, rough-and-tumble fighting found fertile soil.⁵⁹

Just as the economy of the southern backcountry remained locally oriented, the rough-and-tumblers were local heroes, renowned in their communities. There was no professionalization here. Men fought for informal village and county titles; the red feather in the champion's cap was pay enough because it marked him as first among his peers. Paralleling the primitive division of labor in backwoods society, boundaries between entertainment and daily life, between spectators and participants, were not sharply drawn. "Bully of the Hill" Ab Gaines from the Big Hatchie Country, Neil Brown of Totty's Bend, Vernon's William Holt, and Smithfield's Jim Willis—all of them were renowned Tennessee fighters, local heroes in their day. Legendary champions were real individuals, tested gang leaders who attained their status by being the meanest, toughest, and most ruthless fighters, who faced disfigurement and never backed down. Challenges were ever present; yesterday's spectator was today's champion, today's champion tomorrow's invalid.⁶⁰

Given the lives these men led, a world view that embraced fearlessness made sense. Hunters, trappers, Indian fighters, and herdsman who knew the smell of warm blood on their hands refused to sentimentalize an environment filled with threatening forces. It was not that backwoodsmen lived in constant danger but that violence was unpredictable. Recreations like cockfighting deadened men to cruelty, and the gratuitous savagery of gouging matches reinforced the daily truth that life was brutal, guided only by the logic of superior nerve, power, and cunning.⁶¹ With families emotionally or physically distant and civil institutions weak, a man's role in the all-male society was defined less by his ability as a breadwinner than by his ferocity. The touchstone of masculinity was unflinching toughness, not chivalry, duty, or piety. Violent sports, heavy drinking, and impulsive pleasure seeking were appropriate for men whose lives were hard, whose futures were unpredictable, and whose opportunities were limited. Gouging champions were group leaders because they embodied the basic values of their peers. The successful rough-and-tumbler proved his manhood by asserting his dominance and rendering his opponent "impotent," as Thomas Ashe put it. And the loser, though literally or symbolically castrated, demonstrated his mettle and maintained his honor.⁶²

Here we begin to understand the travelers' refrain about plain folk degradation.

⁵⁹ Robertson, "Frolics, Fights, and Firewater"; Isaac, *Transformation of Virginia*, 94–98; and Wyatt-Brown, *Southern Honor*, esp. chaps. 2, 3, 6, 11, 13. For a speculative discussion of the transformation of male roles from premodern to modern times, see Peter N. Stearns, *Be a Man: Males in Modern Society* (New York, 1979), chaps. 2, 3.

⁶⁰ Robertson, "Frolics, Fights, and Firewater," 109. Robertson noted that observers were so aroused during an 1816 fight in Elkton, Tennessee, that several rough-and-tumbles quickly commenced.

⁶¹ Bruce, *Violence and Culture*, chap. 9. For a fine discussion of changing English attitudes toward animals, see Keith Thomas, *Man and the Natural World* (London, 1983), esp. chaps. 3, 4. An American counterpart to Thomas's work remains to be written.

⁶² Moore described the group values of these men; *Frontier Mind*, 119–22. Intense loyalties and frightful intragroup competition at first seem contradictory. For an excellent discussion of how mutually exclusive norms, such as dependence and independence, coexist in the southern mountains, see Kai T. Erikson, *Everything in Its Path* (New York, 1976), 88–93. Also see Stearn's discussion of male values in hunting societies; *Be a Man*, chap. 2.

Setting out from northern ports, whose inhabitants were increasingly possessed by visions of godly perfection and material progress, they found southern upcountry people slothful and backward. Ashe's Quaker friend in Wheeling, Virginia, made the point.⁶³ For Quakers and northern evangelicals, labor was a means of moral self-testing, and earthly success was a sign of God's grace, so hard work and steady habits became acts of piety. But not only Yankees endorsed sober restraint. A growing number of southern evangelicals also embraced a life of decorous self-control, rejecting the hedonistic and self-assertive values of old. During the late eighteenth century, as Rhys Isaac has observed, many plain folk disavowed the hegemonic gentry culture of conspicuous display and found individual worth, group pride, and transcendent meaning in religious revivals. By the antebellum era, new evangelical waves washed over class lines as rich and poor alike forswore such sins as drinking, gambling, cursing, fornication, horse racing, and dancing. But conversion was far from universal, and, for many in backcountry settlements like Wheeling, the evangelical idiom remained a foreign tongue. Men worked hard to feed themselves and their kin, to acquire goods and status, but they lacked the calling to prove their godliness through rigid morality. Salvation and self-denial were culturally less compelling values, and the barriers against leisure and self-gratification were lower here than among the converted.⁶⁴

Moreover, primitive markets and the semisubsistence basis of upcountry life limited men's dependence on goods produced by others and allowed them to maintain the irregular work rhythms of a precapitalist economy. The material base of backwoods life was ill suited to social transformation, and the cultural traditions of the past offered alternatives to rigid new ideals. Closing up shop in mid-week for a fight or horse race had always been perfectly acceptable, because men labored so that they might indulge the joys of the flesh. Neither a compulsive need to save time and money nor an obsession with progress haunted people's imaginations. The backcountry folk who lacked a bourgeois or Protestant sense of duty were little disturbed by exhibitions of human passions and were resigned to violence as part of daily life. Thus, the relative dearth of capitalistic values (such as delayed gratification and accumulation), the absence of a strict work ethic, and a cultural tradition that winked at lapses in moral rigor limited society's demands for sober self-control.⁶⁵

Not just unconverted poor whites but also large numbers of the slave-holding gentry still lent their prestige to a regional style that favored conspicuous displays of leisure. As C. Vann Woodward has pointed out, early observers, such as Robert Beverley and William Byrd, as well as modern-day commentators, have described a distinctly "southern ethic" in American history. Whether judged positively as leisure

⁶³ Ashe, *Travels in America*, 82–88. Also see Williams, *Vogues in Villainy*, 26.

⁶⁴ Isaac, *Transformation of Virginia*, pt. 2; and Donald G. Matthews, *Religion in the Old South* (Chicago, 1977), chap. 3. For an exceptional discussion of the social and religious origins of the northern work ethic, see Daniel T. Rodgers, *The Work Ethic in Industrial America, 1850–1920* (Chicago, 1979), esp. chap. 1. On crime patterns and social differences in the nineteenth-century North and South, see Hindus, *Prison and Plantation*, 34–36, 53–55, 96–98, 242–55; and Ayers, *Vengeance and Justice*, 16–33, 119–23.

⁶⁵ Wyatt-Brown, *Southern Honor*, chaps. 2, 3; Rodgers, *Work Ethic in Industrial America*, chap. 1; Hahn, *Southern Populism*, pt. 1; Ayers, *Vengeance and Justice*, chap. 4; Thornton, *Politics and Power*, chaps. 1–5; and Barney, *Secessionist Impulse*, chap. 1.

or negatively as laziness, the southern sensibility valued free time and rejected work as the consuming goal of life. Slavery reinforced this tendency, for how could labor be an unmitigated virtue if so much of it was performed by despised black bondsmen? When southerners did esteem commerce and enterprise, it was less because piling up wealth contained religious or moral value than because productivity facilitated the leisure ethos. Southerners could therefore work hard without placing labor at the center of their ethical universe. In important ways, then, the upland folk culture reflected a larger regional style.⁶⁶

Thus, the values, ideas, and institutions that rapidly transformed the North into a modern capitalist society came late to the South. Indeed, conspicuous display, heavy drinking, moral casualness, and love of games and sports had deep roots in much of Western culture. As Woodward has cautioned, we must take care not to interpret the southern ethic as unique or aberrant. The compulsions to subordinate leisure to productivity, to divide work and play into separate compartmentalized realms, and to improve each bright and shining hour were the novel ideas. The southern ethic anticipated human evil, tolerated ethical lapses, and accepted the finitude of man in contrast to the new style that demanded unprecedented moral rectitude and internalized self-restraint.⁶⁷

THE AMERICAN SOUTH ALSO SHARED with large parts of the Old World a taste for violence and personal vengeance. Long after the settling of the southern colonies, powerful patriarchal clans in Celtic and Mediterranean lands still avenged affronts to family honor with deadly feuds.⁶⁸ Norbert Elias has pointed out that postmedieval Europeans routinely spilled blood to settle their private quarrels. Across classes, the story was the same:

Two associates fall out over business; they quarrel, the conflict grows violent; one day they meet in a public place and one of them strikes the other dead. An innkeeper accuses another of stealing his clients; they become mortal enemies. Someone says a few malicious words about another; a family war develops. . . . Not only among the nobility were there family vengeance, private feuds, vendettas. . . . The little people too—the hatters, the tailors, the shepards—were all quick to draw their knives.

Emotions were freely expressed: jollity and laughter suddenly gave way to belligerence; guilt and penitence coexisted with hate; cruelty always lurked nearby. The modern middle-class individual, with his subdued, rational, calculating ways, finds it hard to understand the joy sixteenth-century Frenchmen took in ceremonially burning alive one or two dozen cats every Midsummer Day or the pleasure

⁶⁶ Woodward, "Southern Ethic in a Puritan World," 36–37, 42. For a fine analysis of southerners' historic compulsion to discuss their region, see Fred Hobson, *Tell About the South: The Southern Rage to Explain* (Baton Rouge, 1983).

⁶⁷ For Old World examples, see Robert W. Malcolmson, *Popular Recreations in English Society, 1700–1850* (Cambridge, 1973), chap. 4; and Peter Burke, *Popular Culture in Early Modern Europe* (New York, 1978), chaps. 7–9.

⁶⁸ Wyatt-Brown, *Southern Honor*, 366–69; and Julio Caro-Baroja, "Honour and Shame: An Historical Account of Several Conflicts," trans. R. Johnson, in J. G. Peristiany, *Honour and Shame* (Chicago, 1966), 88–91.

eighteenth-century Englishmen found in watching trained dogs slaughter each other.⁶⁹

Despite enormous cultural differences, inhabitants of the southern uplands exhibited characteristics of their forebears in the Old World. The Scots-Irish brought their reputation for ferocity to the backcountry, but English migrants, too, had a thirst for violence. Central authority was weak, and men reserved the right to settle differences for themselves. Vengeance was part of daily life. Drunken hilarity, good fellowship, and high spirits, especially at crossroads taverns, suddenly turned to violence. Traveler after traveler remarked on how forthright and friendly but quick to anger the backcountry people were. Like their European ancestors, they had not yet internalized the modern world's demand for tight emotional self-control.⁷⁰

Above all, the ancient concept of honor helps explain this shared proclivity for violence. According to the sociologist Peter Berger, modern men have difficulty taking seriously the idea of honor. American jurisprudence, for example, offers legal recourse for slander and libel because they involve material damages. But insult—publicly smearing a man's good name and besmirching his honor—implies no palpable injury and so does not exist in the eyes of the law. Honor is an intensely social concept, resting on reputation, community standing, and the esteem of kin and compatriots. To possess honor requires acknowledgment from others; it cannot exist in solitary conscience. Modern man, Berger has argued, is more responsive to dignity—the belief that personal worth inheres equally in each individual, regardless of his status in society. Dignity frees the evangelical to confront God alone, the capitalist to make contracts without customary encumbrances, and the reformer to uplift the lowly. Naked and alone man has dignity; extolled by peers and covered with ribbons, he has honor.⁷¹

Anthropologists have also discovered the centrality of honor in several cultures. According to J. G. Peristiany, honor and shame often preoccupy individuals in small-scale settings, where face-to-face relationships predominate over anonymous or bureaucratic ones. Social standing in such communities is never completely secure, because it must be validated by public opinion whose fickleness compels men constantly to assert and prove their worth. Julian Pitt-Rivers has added that, if

⁶⁹ Elias, *The Civilizing Process*, trans. Edmund Jephcott (New York, 1978), 200–04. While I find dubious Elias's argument on the recent evolution of man's brain, his contention that manners, customs, and etiquette become vehicles of status emulation is convincing. For brilliant recent discussions of routine cruelty to animals through the eighteenth century and beyond, see Thomas, *Man and the Natural World*, chaps. 3, 4; and Robert Darnton, *The Great Cat Massacre* (New York, 1984), chap. 2. For violence in British sports, see Malcolmson, *Popular Recreations*, chap. 3; and Eric Dunning and Kenneth Sheard, *Barbarians, Gentlemen, and Players: A Sociological Study of the Development of Rugby Football* (New York, 1979), 32–43.

⁷⁰ Charles Augustus Murray was especially struck by the quixotic character of Kentucky hunters. See Murray, *Travels in North America* (1840; reprint, London, 1854), 175–76. Grady McWhiney has kindly lent me a draft chapter entitled "Violence" from his forthcoming book on the Celtic origins of southern culture (coauthored with Forrest McDonald). McWhiney's manuscript vividly captures the raw texture of antebellum southern life. I am not persuaded, however, by the Celtic thesis because too much evidence, including works cited above, indicates that violence was endemic to much of Britain and the Continent. Moreover, even if descendants of Celts were more violent than others, class, not culture, may be the reason—a factor McWhiney fails to consider. For other examples of violence, see Darnton, *Great Cat Massacre*, chap. 1; and Lawrence Stone, *The Crises of the Aristocracy, 1558–1642* (New York, 1965), 223–34.

⁷¹ Berger et al., *Homeless Mind*, 83–94.

society rejects a man's evaluation of himself and treats his claim to honor with ridicule or contempt, his very identity suffers because it is based on the judgment of peers. Shaming refers to that process by which an insult or any public humiliation impugns an individual's honor and thereby threatens his sense of self. By risking injury in a violent encounter, an affronted man—whether victorious or not—restores his sense of status and thus validates anew his claim to honor. Only valorous action, not words, can redeem his place in the ranks of his peer group.⁷²

Bertram Wyatt-Brown has argued that this Old World ideal is the key to understanding southern history. Across boundaries of time, geography, and social class, the South was knit together by a primal concept of male valor, part of the ancient heritage of Indo-European folk cultures. Honor demanded clan loyalty, hospitality, protection of women, and defense of patriarchal prerogatives. Honorable men guarded their reputations, bristled at insults, and, where necessary, sought personal vindication through bloodshed. The culture of honor thrived in hierarchical rural communities like the American South and grew out of a fatalistic world view, which assumed that pain and suffering were man's fate. It accounts for the pervasive violence that marked relationships between southerners and explains their insistence on vengeance and their rejection of legal redress in settling quarrels. Honor tied personal identity to public fulfillment of social roles. Neither bourgeois self-control nor internalized conscience determined status; judgment by one's fellows was the wellspring of community standing.⁷³

In this light, the seemingly trivial causes for brawls enumerated as early as Fithian's time—name calling, subtle ridicule, breaches of decorum, displays of poor manners—make sense. If a man's good name was his most important possession, then any slight cut him deeply. "Having words" precipitated fights because words brought shame and undermined a man's sense of self. Symbolic acts, such as buying a round of drinks, conferred honor on all, while refusing to share a bottle implied some inequality in social status. Honor inhered not only in individuals but also in kin and peers; when members of two cliques had words, their tested leaders or several men from each side fought to uphold group prestige. Inheritors of primal honor, the southern plain folk were quick to take offense, and any perceived

⁷² Honor has especially concerned anthropologists of the Mediterranean. In Peristiany's *Honour and Shame*, see, especially, "Introduction," 9–18; Pitt-Rivers, "Honour and Social Status," 19–77; and Caro-Baroja, "Honor and Shame," 81–137. All of the essays in this collection are informative. Also see Pitt-Rivers, *The Fate of Sechem or the Politics of Sex* (Cambridge, 1977), "Honor," in David Sills, ed., *The International Encyclopedia of the Social Sciences*, 6 (New York, 1968), 503–10; and J. Davis, *People of the Mediterranean* (London, 1977), 89–101.

⁷³ Wyatt-Brown's book is brilliant but occasionally exasperating. For example, he too often treated culture as something established millenia ago and barely modified until the nineteenth century—thus, his rather cavalier dismissal of slavery as the major formative fact of southern history. In his urge to trace broad themes, he sometimes oversimplified complex cultural diversity. Amid marvelous ethnographic detail and insightful analyses of kinship patterns, sexual roles, power relationships, and so forth, Wyatt-Brown frequently fell back on a static and superorganic concept of culture that failed to do justice to those immediate historical changes, particularities of social life, and specific material conditions that shape values, beliefs, and ideologies. Southerners were not Teutonic tribesmen; they were not even Celtic herdsmen. Thus, the concept of honor, as applied by Wyatt-Brown, is too all-encompassing. We need to know more about how and why honor varies across economic systems and cultures. Nevertheless, Wyatt-Brown asked the right questions, and his book is filled with probing discussions and brilliant insights. It is a seminal work because it will frame years of future debate. Orlando Patterson has written a penetrating critique of Wyatt-Brown's book. See *Reviews in American History*, 12 (1984): 24–30. For a probing discussion of the northern conscience versus southern shame, see Ayers, *Vengeance and Justice*, chap. 1.

affront forced a man either to devalue himself or to strike back violently and avenge the wrong.⁷⁴

The concept of male honor takes us a long way toward understanding the meaning of eye-gouging matches. But backwoods people did not simply acquire some primordial notion without modifying it. Definitions of honorable behavior have always varied enormously across cultures. The southern upcountry fostered a particular style of honor, which grew out of the contradiction between equality and hierarchy. Honorific societies tend to be sharply stratified. Honor is apportioned according to rank, and men fight to maintain personal standing within their social categories. Because black chattel slavery was the basis for the southern hierarchy, slave owners had the most wealth and honor, while other whites scrambled for a bit of each, and bondsmen were permanently impoverished and dishonored.⁷⁵ Here was a source of tension for the plain folk. Men of honor shared freedom and equality; those denied honor were implicitly less than equal—perilously close to a slave-like condition. But in the eyes of the gentry, poor whites as well as blacks were outside the circle of honor, so both groups were subordinate. Thus, a herdsman's insult failed to shame a planter since the two men were not on the same social level. Without a threat to the gentleman's honor, there was no need for a duel; horsewhipping the insolent fellow sufficed.⁷⁶

Southern plain folk, then, were caught in a social contradiction. Society taught all white men to consider themselves equals, encouraged them to compete for power and status, yet threatened them from below with the specter of servitude and from above with insistence on obedience to rank and authority.⁷⁷ Cut off from upper-class tests of honor, backcountry people adopted their own. A rough-and-tumble was more than a poor man's duel, a botched version of genteel combat. Plain folk chose not to ape the dispassionate, antiseptic, gentry style but to invert it. While the gentleman's code of honor insisted on cool restraint, eye gougers gloried in unvarnished brutality. In contrast to duelists' aloof silence, backwoods fighters screamed defiance to the world. As their own unique rites of honor, rough-and-tumble matches allowed backcountry men to shout their equality at each other. And eye-gouging fights also dispelled any stigma of servility. Ritual boasts, soaring oaths, outrageous ferocity, unflinching bloodiness—all proved a man's freedom. Where the slave acted obsequiously, the backwoodsman resisted the slightest affront; where human chattels accepted blows and never raised a hand, plain folk

⁷⁴ Wyatt-Brown, *Southern Honor*, chaps. 2, 3, 13. Also see Bruce, *Violence and Culture*, chaps. 1–4; Isaac, *Transformation of Virginia*, chap. 5; Ayers, *Vengeance and Justice*, 9–28, 99–101; and William J. Cooper, *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978), 69–74, 238–44. Growing numbers of evangelicals, a small bourgeois class, and transplanted foreigners and Yankees were the most conspicuous opponents of the southern concept of honor.

⁷⁵ By definition, bondsmen were "men without honor" in all slave societies. Orlando Patterson, *Slavery and Social Death* (Cambridge, Mass., 1982), 10–13, and chap. 3.

⁷⁶ Williams, *Duelling in the Old South*, 26–28; and Wyatt-Brown, *Southern Honor*, 355–57.

⁷⁷ On the ambiguous position of poor whites with respect to slavery and equality, see William L. Barney, *The Road to Secession: A New Perspective on the Old South* (New York, 1972), 10–11, 42–43, 62–65, 136–37; Barney, *Secessionist Impulse*, 38–48; Frederickson, *Black Image*, chap. 2; Cooper, *The South and the Politics of Slavery*, 370–74; Thornton, *Politics and Power*, xviii–xx, 55–58, 320–21, 443–50; Morgan, *American Slavery*, 376–87; and Patterson, *Slavery and Social Death*, 94–97. Ayers, unlike Wyatt-Brown, argued that slavery was essential to the southern honor ethic, a position I find persuasive; *Vengeance and Justice*, 26–27.

celebrated violence; where blacks could not jeopardize their value as property, poor whites proved their autonomy by risking bodily parts. Symbolically reaffirming their claims to honor, gouging matches helped resolve painful uncertainties arising out of the ambiguous place of plain folk in the southern social structure.⁷⁸

Backwoods fighting reminds us of man's capacity for cruelty and is an excellent corrective to romanticizing premodern life. But a close look also keeps us from drawing facile conclusions about innate human aggressiveness. Eye gouging represented neither the "real" human animal emerging on the frontier, nor nature acting through man in a Darwinian struggle for survival, nor anarchic disorder and communal breakdown. Rather, rough-and-tumble fighting was ritualized behavior—a product of specific cultural assumptions. Men drink together, tongues loosen, a simmering old rivalry begins to boil; insult is given, offense taken, ritual boasts commence; the fight begins, mettle is tested, blood redeems honor, and equilibrium is restored. Eye gouging was the poor and middling whites' own version of a historical southern tendency to consider personal violence socially useful—indeed, ethically essential.⁷⁹

ROUGH-AND-TUMBLE FIGHTING EMERGED from the confluence of economic conditions, social relationships, and culture in the southern backcountry. Primitive markets and the semisubsistence basis of life threw men back on close ties to kin and community. Violence and poverty were part of daily existence, so endurance, even callousness, became functional values. Loyal to their localities, their occupations, and each other, men came together and found release from life's hardships in strong drink, tall talk, rude practical jokes, and cruel sports. They craved one another's recognition but rejected genteel, pious, or bourgeois values, awarding esteem on the basis of their own traditional standards. The glue that held men together was an intensely competitive status system in which the most prodigious drinker or strongest arm wrestler, the best tale teller, fiddle player, or log roller, the most daring gambler, original liar, skilled hunter, outrageous swearer, or accurate marksman was accorded respect by the others. Reputation was everything, and scars were badges of honor. Rough-and-tumble fighting demonstrated unflinching willingness to inflict pain while risking mutilation—all to defend one's standing among peers—and became a central expression of the all-male subculture.

⁷⁸ On dueling, see Bruce, *Violence and Culture*, chap. 1; Williams, *Duelling in the Old South*, chaps. 1–4; Wyatt-Brown, *Southern Honor*, 350–61; Hindus, *Prison and Plantation*, 42–48; and Stephen M. Stowe, "The Touchiness of the Gentleman Planter: The Sense of Esteem and Continuity in the Ante-Bellum South," *Psychohistory Review*, 8 (1979): 6–17. Symbolic inversion has received considerable attention recently from anthropologists. For a wide-ranging sample of its applications, see Barbara A. Babcock, ed., *The Reversible World: Symbolic Inversion in Art and Society* (Ithaca, 1978). By the antebellum period, knife fights with opponents' arms tied together and quickdraw gun battles marked backwoods fighting, which again seems to mock, with outrageous brutality, the decorum of duels. See Williams, *Duelling in the Old South*, 7; and Davis, *Homicide in American Fiction*, 267–72.

⁷⁹ Rohrbaugh documented the unusually high level of violence on the southwestern frontier in contrast to the northwestern edge of settlement; *Trans-Appalachian Frontier*, 117–18, 275–84. Also see Sheldon Hackney, "Southern Violence," *AHR*, 74 (1969): 906–25; Bruce, *Violence and Culture*; Wyatt-Brown, *Southern Honor*, chap. 13; Isaac, *Transformation of Virginia*, chap. 5; Hindus, *Prison and Plantation*, chap. 3; Ayers, *Vengeance and Justice*, 98–101, 113–17; Williams, *Vogues in Villainy*, 31–38; Franklin, *Militant South*, chap. 3; and McWhiney, "Violence." For violence in the contemporary South, see John Shelton Reed, *The Enduring South* (Lexington, Mass., 1972), chap. 5.

Eye gouging continued long after the antebellum period. As the market economy absorbed new parts of the backcountry, however, the way of life that supported rough-and-tumbling waned. Certainly by mid-century the number of incidents declined, precisely when expanding international demand brought ever more upcountry acres into staple production.⁸⁰ Towns, schools, churches, revivals, and families gradually overtook the backwoods. In a slow and uneven process, keelboats gave way to steamers, then railroads; squatters, to cash crop farmers; hunters and trappers, to preachers. The plain folk code of honor was far from dead, but emergent social institutions engendered a moral ethos that warred against the old ways. For many individuals, the justifications for personal violence grew stricter, and mayhem became unacceptable.⁸¹

Ironically, progress also had a darker side. New technologies and modes of production could enhance men's fighting abilities. "Birmingham and Pittsburgh are obliged to complete . . . the equipment of the 'chivalric Kentuckian,'" Charles Augustus Murray observed in the 1840s, as bowie knives ended more and more rough-and-tumbles. Equally important, in 1835 the first modern revolver appeared, and manufacturers marketed cheap, accurate editions in the coming decade. Dueling weapons had been costly, and Kentucky rifles or horse pistols took a full minute to load and prime. The revolver, however, which fitted neatly into a man's pocket, settled more and more personal disputes. Raw and brutal as rough-and-tumbling was, it could not survive the use of arms. Yet precisely because eye gouging was so violent—because combatants cherished maimings, blindings, even castrations—it unleashed death wishes that invited new technologies of destruction.⁸²

With improved weaponry, dueling entered its golden age during the antebellum era. Armed combat remained both an expression of gentry sensibility and a mark of social rank. But in a society where status was always shifting and unclear, dueling did not stay confined to the upper class. The habitual carrying of weapons, once considered a sign of unmanly fear, now lost some of its stigma. As the backcountry changed, tests of honor continued, but gunplay rather than fighting tooth-and-nail appealed to new men with social aspirations.⁸³ Thus, progress and technology slowly circumscribed rough-and-tumble fighting, only to substitute a deadlier option. Violence grew neater and more lethal as men checked their savagery to murder each other.

⁸⁰ "The next thing I knowed I was a cornen down on him with my hands and my teeth like when I was young, fighten back home. I recollect a thinken, 'If I cain't kill him, I'll mark him up good.' So's I gouged at his eye chewed on his yer. I'd know him now in a million." Thus, Clovis Nevels described a fight in the 1940s in Harriet Arnow's novel *The Dollmaker* (New York, 1954). The contemporary works of novelist Harry Crews also contain descriptions of mayhem resembling eye-gouging matches.

⁸¹ Leyburn, *Scotch Irish*, 264–67; Hahn, *Roots of Populism*, chaps. 1, 2, 4, 5; Barney, *Secessionist Impulse*, chap. 1; Ayers, *Vengeance and Justice*, conclusion; and Thornton, *Politics and Power*, 291–311, 318–21.

⁸² Murray, *Travels in North America*, 175–78; Hollon, *Frontier Violence*, 109–10; Franklin, *Militant South*, chap. 3; Wyatt-Brown, *Southern Honor*, 350–61; Bruce, *Violence and Culture*, chap. 1; Davis, *Homicide in American Fiction*, chap. 10; McWhiney, "Violence"; and Dorson, *Davy Crockett*, 84.

⁸³ Johnson, *Antebellum North Carolina*, 46–47; and Bruce, *Violence and Culture*, chap. 1. Wyatt-Brown succinctly captured the social function of the code of honor: "Duelling was a means to demonstrate status and manliness among those calling themselves gentlemen, whether born of noble blood or not"; *Southern Honor*, 355. Williams also saw dueling as symbolic social climbing; *Duelling in the Old South*, chap. 3.

American Marshall Planners and the Search for a European Neocapitalism

MICHAEL J. HOGAN

IN THE LAST DECADE OR SO, NEW LEFT, NEW RIGHT, organizational, and institutional historians alike have begun to see the emergence of a corporative capitalism as one of the most important developments in recent American history. In the Progressive-era policies of the National Civic Federation, in Herbert Hoover's brand of associationalism, in the business-government commonwealth promoted by many New Dealers, and in the political economy of the Eisenhower years, they have traced the development of an integrated economic system and of new patterns of collaboration and interpenetration between private groups and government authorities. In this system, group conflict and natural market forces are tempered by institutional mechanisms of regulation and control and by a host of cooperating committees and other voluntary frameworks through which public and private leaders share responsibility for economic policy and management. In theory, at least, the resulting fusion of market forces and institutional controls, of private and public power, works to preserve the economic and political advantages of an older, more competitive capitalism, while avoiding the threat to private enterprise that supposedly inheres in an unregulated pursuit of self-interest or in an oppressive and paternalistic statism.¹

The existing literature, however, says little about the international dimension of this evolving neocapitalism. It tells us little about how American leaders have applied their corporative strategies to the management of foreign policy and the organization of the international system or about the specific institutional mechanisms through which they have worked to integrate economies and blend public and private power in ways that would maximize the chances for social peace and

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¹ James Weinstein, *The Corporate Ideal in the Liberal State* (Boston, 1968); Joan Hoff-Wilson, *Herbert Hoover: Forgotten Progressive* (Boston, 1975); Ellis W. Hawley, "Herbert Hoover, the Commerce Secretariat, and the Vision of an 'Associative State,'" *Journal of American History*, 61 (1974-75): 116-40, *The Great War and the Search for a Modern Order* (New York, 1979), and *The New Deal and the Problem of Monopoly: A Study in Economic Ambivalence* (Princeton, 1966); and Robert Griffith, "Dwight D. Eisenhower and the Corporate Common-

productive efficiency.² An examination of the Marshall Plan can tell us more than we now know about this facet of public policy. Through the Marshall Plan, American leaders sought to recast Europe in the image of American neocapitalism. They envisioned a Western European system in which class conflict would give way to corporative collaboration, economic self-sufficiency to economic interdependence, international rivalry to rapprochement and cooperation, and arbitrary national controls to the integrating powers of supranational authorities and natural market forces. One line of their policy aimed at liberalizing trade and making currencies convertible, another at forging national and transnational networks of private cooperation and public-private power sharing, and a third at building central institutions of coordination and control. Through these and related initiatives American Marshall planners hoped to create an integrated European market—one that could absorb German power, boost productivity, raise living standards, lower prices, and thus set the stage for security and recovery on the Continent and for a fully multilateral system of world trade.

THE IDEA OF EUROPEAN ECONOMIC INTEGRATION first appeared in American policy planning in the early months of 1947. By then it was clear that previous programs of piecemeal assistance had failed to bring economic and political stability to Europe. Economic conditions there actually worsened in the wake of the winter crisis of 1946–47, and this combined with political turmoil in France and Italy, failure of Allied leaders to agree on terms for Germany's unification, and fear that Soviet policy would gain from confusion and weakness in the West led American officials to consider a major foreign aid initiative. Their planning now envisioned a

wealth," *AHR*, 87 (1982): 87–122. Also see Hawley, "The Discovery and Study of Corporate Liberalism," *Business History Review*, 52 (1978): 309–20; Kim McQuaid, "Corporate Liberalism and the American Business Community," *ibid.*, 342–67; and Robert M. Collins, *The Business Response to Keynes, 1929–1964* (New York, 1981). As Griffith explained in the essay cited above, historians use the terms "corporatism," "corporate liberalism," and "neo-capitalism" interchangeably to describe "both the historical process through which liberal institutions have adapted to the imperatives of large-scale organization and the ideology that has been used to promote and legitimize such a process"; "Dwight D. Eisenhower and the Corporate Commonwealth," 97 n. 21. The historical advocates of neocapitalism differ on important questions, particularly on the proper balance between public and private power, and this balance has tended to shift over the course of the twentieth century. Nevertheless, neocapitalists differ from both laissez-faire and welfare state theorists in that they accept a positive role for the state in such areas as social welfare and economic regulation, while seeking to contain this role by organizing public-private power-sharing arrangements and voluntary frameworks of self-regulation, enlightened group action, and intergroup cooperation. For a general discussion of the continuities and changes in the neocapitalist approach to domestic and foreign economic policy over the course of this century, see my "Revival and Reform: America's Twentieth-Century Search for a New Economic Order Abroad," *Diplomatic History*, 8 (1984): 287–310.

² For efforts in this direction, see Hoff-Wilson, *Herbert Hoover*; Hawley, *Great War*; Griffith, "Dwight D. Eisenhower and the Corporate Commonwealth"; Michael J. Hogan, *Informal Entente: The Private Structure of Cooperation in Anglo-American Economic Diplomacy, 1918–1928* (Columbia, Mo., 1977); "Thomas W. Lamont and European Recovery: The Diplomacy of Privatism in a Corporatist Age," in Kenneth Paul Jones, ed., *U. S. Diplomats in Europe, 1919–1941* (Santa Barbara, Calif., 1981), 5–22, and "Revival and Reform"; and Melvyn P. Leffler, *The Elusive Quest: America's Pursuit of European Stability and French Security, 1919–1933* (Chapel Hill, 1979). Also see Thomas J. McCormick, "Drift or Mastery? A Corporatist Synthesis for American Diplomatic History," *Reviews in American History*, 10 (1982): 318–30. On the European side, see, especially, Charles S. Maier, "The Two Postwar Eras and the Conditions for Stability in Twentieth-Century Western Europe," *AHR*, 86 (1981): 327–52, and *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton, 1975).

comprehensive recovery program for Europe as a whole, one based on the principles of collective action, joint programming, and resource sharing and one aimed at breaking bottlenecks to production, making currencies convertible, reducing trade barriers, and integrating economies.

Such an approach, it was assumed, could eliminate the territorial constraints, government restrictions, and bilateral trade and payments agreements that prevented the most efficient use of resources, hampered productivity, and slowed the pace of recovery in Europe. It could also fortify the Western democracies against communist attack, forge a collective framework for controlling German nationalism, and, in this way, reconcile the revival of Germany's productive power with the security and economic needs of its former victims. This was the thinking embodied in the public and private pronouncements of American policy makers on the eve of Secretary of State George C. Marshall's celebrated commencement address at Harvard University on June 5, 1947. Marshall himself was "deeply sympathetic" to the idea of European integration, and, although he thought that European leaders should take the initiative in devising a recovery program, the emphasis in his Harvard speech on a comprehensive scheme and on collective responsibility, mutual aid, and "joint" action in "Europe as a whole" was designed to point the Europeans in the right direction.³

The American position became even clearer in the summer and fall of 1947, when the Committee on European Economic Cooperation (CEEC) met in Paris to draw up a recovery program. American leaders urged the sixteen countries represented to draft proposals for mutual aid and resource sharing and to establish procedures for the collective screening of individual country requirements and the coordination of national investment decisions on an area-wide basis. They then worked with their European counterparts to devise a plan that acknowledged these principles and procedures, called for a "new era of European economic cooperation," and promised "concerted action" to maximize production, reduce tariffs, and stabilize currencies. American policy makers, according to a CEEC delegation that visited Washington in October, believed that such measures could "bring benefits to Europe through the creation of a larger domestic market and [the] concentration of productive effort." Indeed, the aid proposal that the Truman administration submitted to Congress in December and the Foreign Assistance Act that Congress passed the following spring urged Europeans to replace the old "self-defeating" pattern of "narrow nationalism" with new forms of economic cooperation and integration. This, they agreed, was the path to "lasting peace and prosperity" on the Continent.⁴

³ For Marshall's sympathetic view of European union, see Marshall to Senator Arthur H. Vandenberg, June 4, 1947, Department of State *Bulletin*, June 22, 1947, p. 1213. Marshall's speech at Harvard is printed in U.S., Department of State, *Foreign Relations of the United States* [hereafter, *FRUS*], 1947, 3 (Washington, D.C., 1972), 237–39. For a general discussion of American policy planning see Michael J. Hogan, "The Search for a 'Creative Peace': The United States, European Unity, and the Origins of the Marshall Plan," *Diplomatic History*, 6 (1982): 267–85.

⁴ For the CEEC's report, see Committee on European Economic Cooperation, *General Report*, 1, and *Technical Reports*, 2 (London, 1947). For the remark by the CEEC delegation in Washington, see its telegram to the participating governments, October 31, 1947, in *FRUS*, 1947, 3: 456–61. For the quotations from the administration's aid proposal and the Foreign Assistance Act, see Ernst H. Van Der Beugel, *From Marshall Aid to*

Thereafter, policy makers in the State Department and in the Economic Cooperation Administration (ECA), the American agency established to administer the European Recovery Program (ERP), employed a variety of strategies to integrate Europe and create the new era of "lasting peace and prosperity." One such strategy involved the ECA's decision to make Marshall Plan countries, through their Organization for European Economic Cooperation (OEEC), collectively responsible for reviewing national requirements and integrating these into consolidated annual recovery programs that would include plans for raising production, liberalizing trade, placing government finances on a sound basis, and making collective use of European resources. Any program for putting Europe on a self-supporting basis, as ECA Administrator Paul G. Hoffman told the OEEC in July 1948, could not be "traced on an old design" or "brought about by old ways of doing business," by "old concepts of how a nation's interests are best served," or by "old separatist lines" of economic activity. It would require "new patterns of intra-European trade and exchange," new efforts to adjust national economies to the needs of "Europe as a whole," and "new directions in the use of Europe's resources."⁵

These should include Germany's resources as well as those of the other participating countries. The need, according to this line of American strategy, was for a balanced recovery program that would incorporate western Germany into a "strong common structure of free Europeanism." A policy that "segregated" Germany might lead to the "rebirth of aggressive German nationalism," German economic domination of the Continent, or a dangerous Soviet-German rapprochement. But one that fitted Germany into the framework of a "general European union" could capture its resources for the West, aid European recovery, benefit the "strategic position" and reduce the "security preoccupations of the Western powers," and encourage the growth of democratic politics in the former Reich. If Germany could not, for political reasons, be made a member of the Brussels Pact and North Atlantic Treaty, its resources could at least form part of a larger Western European economic system.⁶

Another strategy centered on the so-called counterpart funds, the local currency equivalent of American grants set aside by participating countries for use under terms approved by the ECA. The agency employed these funds to restrain inflation where this would revive "internal production and foreign trade" and to underwrite

Atlantic Partnership: European Integration as a Concern of American Foreign Policy (New York, 1966), 106; and U.S. Congress, House of Representatives, Committee on International Relations, Historical Series, *Selective Executive Hearings, Foreign Economic Assistance Programs*, part 1: *Foreign Assistance Act of 1948* (Public Law 472), 80th Congress, 2d sess., 1948 (Washington, D.C., 1976), 254–72 [hereafter, House, *Foreign Assistance Act of 1948*]. For additional details on this part of the story, see my "Paths to Plenty: American Marshall Planners and the Idea of European Economic Integration, 1947–1948," *Pacific Historical Review*, 53 (1984): 337–66.

⁵ Hoffman, as quoted in W. Averell Harriman to ECA, July 25, 1948, Washington, D.C., Agency for International Development Records, ECA Telegram File [hereafter, ECA File], Acc. 53A278, box 5.

⁶ See the State Department paper of March 31, 1949, attached to Secretary of State Dean Acheson's memorandum to President Truman, March 31, 1949; the paper of March 23, 1949, prepared by Robert Murphy of the State Department's Office of German and Austrian Affairs; the State Department's undated annex and appendix to Murphy's paper; and the memoranda of February 7 and March 8, 1949, by George Kennan, director of the State Department's Policy Planning Staff, *FRUS*, 1949, 3 (Washington, D.C., 1974), 142–55, 118–37, 90–93, 96–102.

capital investment where this would enhance “national productivity” and contribute to recovery in “Europe as a whole.”⁷ Initially, it used counterpart funds to control inflationary pressures, for example, by enabling the British government to reduce short-term public debt and encouraging the Italian and French governments to balance budgets, limit bank credit, and maintain wage and price stability.⁸ Once inflationary pressures seemed under control, however, policy makers in the ECA began discussing a “critical sectors” approach to counterpart financing by which they hoped to direct investment into those industries that had a natural advantage in labor costs or raw materials and could thus compete without government subsidies, protective tariffs, or restrictive arrangements. The result, or so it was expected, would be an integrated European economic system in which freer trade, greater specialization, and more efficient use of resources would work to raise productivity, lower prices, and close the Continent’s trade deficit with the Western Hemisphere.⁹

The same goal guided the ECA’s attack on exchange restrictions, quantitative import quotas, and bilateral trade agreements in Europe. These, too, discouraged the most efficient use of resources. They made participating countries dependent on the United States for goods generally available on the Continent, slowed the pace of European recovery, and hampered efforts to expand trade and increase production. Both the ECA and State Department thus lent support to the formation of a European customs union and encouraged negotiations for a Franco-Italian tariff union and a merger of this union with the Benelux group.¹⁰ In addition, they supported the intra-European payments plans of 1948 and 1949, under which ECA dollars helped creditors finance a system of drawing rights that debtors used to balance their accounts without resorting to quantitative import quotas and other restrictions on trade.¹¹

Before these strategies could work, however, British opposition to European

⁷ Harriman, circular, August 31, 1948, Washington National Records Center, Suitland, Md., Department of State Records, Record Group 84, London Embassy Files [hereafter, RG 84], box 1035, folder 850, Marshall Plan. Also see Hoffman to Harriman, September 14, 1948, Washington, D.C., Department of the Treasury Records [hereafter, Treasury Records], Acc. 66A1039, box 4, Marshall Plan, Local Currency Counterpart folder; and Harry Bayard Price, *The Marshall Plan and Its Meaning* (Ithaca, New York, 1955), 104.

⁸ Price examined policy in France and Britain; *Marshall Plan and Its Meaning*, 104–07. For American policy in Italy and Italian-American discussions over counterpart funds, see Harriman to Hoffman, October 14, 1948; William Foster to Hoffman, December 29, 1948, ECA File, box 5. Also see the ECA aide-memoires of September 30, 1948, resumes of conversations between the ECA mission chief and Italian Premier Alcide de Gasperi, October 1, 12, 1948, and the memorandum by Arthur Marget of the ECA to Harriman, December 21, 1948, in Washington National Records Center, Suitland, Md., Records of the Agency for International Development, Record Group 286 [hereafter, RG 286], Acc. 53A177, box 33, Italy folder.

⁹ Harriman to Hoffman, October 11, 14, November 29, 1949; Harriman to Hoffman, December 20, 1949; Philip Bonsal to Hoffman, December 28, 1949, ECA File, box 8; Hoffman to Harriman, October 12, 1949; Foster to Hoffman, October 12, 1949; Hoffman to Harriman, November 11, 1949, *ibid.*, box 50; and Harriman to Hoffman, December 2, 1949, RG 286, Acc. 53A405, box 50, Finance-Investments folder.

¹⁰ For American support of the European Customs Union Study Commission, see Marshall to the American Embassy, Brussels, March 10, 1948, National Archives, Washington, D.C., Department of State Records, Record Group 59 [hereafter, RG 59], file 640.002/2–448. On the Franco-Italian negotiations, see *ibid.*, file 651.6531/12–947, 1–1248, 1–1648, 1–2448, 1–2848; and Raymond Vernon, memorandum to Paul Nitze, January 14, 1948, *ibid.*, file 840.50 Recovery/1–1448. For the American position on French efforts to merge the Franco-Italian and Benelux groups, see Marshall to the American Legation, Luxembourg, January 29, 1948, *ibid.*, file 655.5631/1–2948.

¹¹ William Diebold, Jr., *Trade and Payments in Western Europe: A Study in Economic Cooperation, 1947–51* (New York, 1952), 34–46, 64–80.

union and French opposition to German integration had to be overcome. The French were reluctant to compromise their security or their Monnet Plan for industrial modernization in order to expedite Germany's revitalization. By 1948 their demands for reparations and proposals for ownership and management of the Ruhr industries, limitations and prohibitions on German production, and political decentralization in the western zones were colliding with American plans to revitalize German industry, reduce Allied occupation controls, and unify the western zones under a new German government.¹² The British, on the other hand, were refusing to submerge their sovereignty in a European economic union at the cost of their control over domestic policies, their ties to the Commonwealth, and their leadership of the sterling bloc. They conditioned their participation in a European customs union on special arrangements to protect the system of imperial preference. They played an indirect role in scuttling French proposals for merging the Benelux group with the proposed Franco-Italian tariff union. And to safeguard their position as leaders of the sterling area, they demanded special consideration for this area in the intra-European payments plans of 1948 and 1949.¹³

The political accommodation that resolved some of these differences in 1949 was offset by stubborn economic problems and ongoing Anglo-American disagreement over how to address them. The Allies agreed to loosen the restrictions on German industry, reduce the number of plants to be dismantled for reparation, establish an international authority to oversee the Ruhr, and replace military rule in the western zones with an Allied high commission of civilian officials.¹⁴ These and other agreements brought French policy more into line with American thinking and set the stage for the formation of the Federal Republic and its subsequent accession to the OEEC and Council of Europe. At the same time, however, European exports to the dollar area began to decline, in part because of economic recession in the United States but also, as the Economic Commission for Europe reported, because of resistance to coordinated economic planning and the desire among European nations for self-sufficiency. Too many Marshall Plan countries were still pursuing policies that prevented gains in specialization, economies of scale, and labor productivity required to drive down prices, make their commodities more competitive, and balance their accounts with the dollar area.¹⁵

The need to redress the trade imbalance, as well as the different British and American strategies for doing so, grew apparent in mid-1949. By then the sterling area's dollar deficit had reached proportions that threatened to drain the Bank of

¹² John Gimbel, *The American Occupation of Germany: Politics and the Military, 1945-1949* (Stanford, 1968), 194-225; John C. Campbell, *The United States in World Affairs, 1948-1949* (New York, 1949), 70-79, 464-75; and Hogan, "Paths to Plenty." I have examined the German question in some detail. See my "European Union and German Integration: Marshall Planners and the Search for Recovery and Security in Western Europe," in Charles S. Maier, ed., *Germany and the Marshall Plan* (forthcoming, 1986).

¹³ For these developments, see RG 59, file 640.002/11-647, 12-1847, 1-1648, 1-1948, 2-1148, 2-2448, 4-2448, and file 655.5631/11-1447, 2-448, 2-848; and Diebold, *Trade and Payments in Western Europe*, 34-46, 64-80.

¹⁴ The Allied agreements came at the Washington Foreign Ministers Conference of late March and early April. On the discussions there and the agreements that resulted, see *FRUS*, 1949, 3: 156-86. Also see Hogan, "European Union and German Integration."

¹⁵ United Nations, Economic Commission for Europe, *Economic Survey of Europe in 1948* (Geneva, 1949), esp. 211-28.

England of its gold and dollar reserves. To stabilize the situation, the Labour government relied on bilateral controls, restrictions on dollar-area imports, and international agreements to bolster the price of sterling-area exports.¹⁶ Such measures were expected to stanch the dollar hemorrhage at the expense of the ECA's plans for an integrated European economic order and a fully multilateral system of world trade. And in response to them, American leaders called for a strategy that was more compatible with these plans and with the findings of the Economic Commission for Europe. They urged a maximum liberalization of trade and payments on the Continent and a substantial devaluation of the pound sterling and other European currencies. Such measures, they said, would improve the dollar position of Marshall Plan countries and create a "single-market Europe" in which "large low-cost production" could help "close the dollar deficit" and lessen the need for discrimination against American exports.¹⁷

Hoffman and other American leaders appealed for British cooperation during Anglo-American financial talks that opened in Washington on September 7. In reality, however, they had left the British with few alternatives to devaluation. The American executive director of the International Monetary Fund (IMF) had already overcome British opposition and won support for a resolution authorizing the IMF to study European exchange rates. He subsequently took charge of a special committee established to investigate the "dollar gap" and drafted a report that called for a downward revision in European exchange rates. The report was almost certain to have the support of the IMF's board of directors when it met in September, and the prospect of this, particularly of the pressure it would put on sterling, made it virtually impossible for the British to avoid devaluation. During the Anglo-American financial talks in Washington, they privately informed the Americans of their decision, and on September 18, six days after the talks had adjourned, they officially announced a 30 percent depreciation of pound sterling.¹⁸

British devaluation ended the sterling crisis, and, by prompting a general devaluation and realignment of other European currencies, it also fulfilled one of

¹⁶ Ambassador Lewis Douglas, London, to Acheson, June 16, 22, 1949; Willard Thorp, assistant secretary of state for economic affairs, memorandum for the secretary of state, June 27, 1949; and Secretary of the Treasury John W. Snyder to Acheson, July 9, 10, 1949, *FRUS*, 1949, 4 (Washington, D.C., 1975), 784–86, 787–90, 793–96, 799–801, 801–02.

¹⁷ Harriman to ECA, August 15, 1949, ECA File, box 8. Also see Acheson to Douglas, June 27, 30, 1949; Hoffman to Acting U.S. Special Representative Milton Katz, August 3, 1949; National Advisory Council on International Monetary and Financial Problems, undated paper, *FRUS*, 1949, 4: 796–97, 797–99, 412–15, 419–21.

¹⁸ J. Burke Knapp, memorandum to Thorp, April 4, 1949, with attached memorandum by Frank Southard, under secretary of the treasury and U.S. executive director, IMF, to Snyder and Assistant Secretary of the Treasury William McC. Martin, Jr., March 31, 1949, RG 59, file 840.50 Recovery/4–449; Acheson to Douglas, April 7, 1949, *ibid.*, Recovery/4–2449; Acheson to certain diplomatic offices, April 12, 1949, *FRUS*, 1949, 4: 382–83; Thomas Finletter, ECA mission chief, London, to Harriman, April 13, 1949, Washington, D.C., W. Averell Harriman Papers [hereafter, Harriman Papers], United Kingdom folder; Southard, memoranda to Snyder and Martin, September 5, 8, 12, 1949, Harry S. Truman Library, Independence, Mo. [hereafter, HSTL], John W. Snyder Papers, box 20, Alphabetical File, International Monetary Fund folder. As these documents point out, Snyder agreed to delay action by the IMF's board of directors until mid-September. It seems likely that he did so, however, only after British officials at the Washington financial talks had informed him of their decision to devalue. Also see Dean Acheson, *Present at the Creation: My Years in the State Department* (New York, 1969), 324–25. For the text of the announcement by Chancellor of the Exchequer Sir Stafford Cripps, see *New York Times*, September 19, 1949, p. 6.

the ECA's requirements for narrowing the dollar gap and integrating European economies. To fulfill its second requirement, a maximum liberalization of intra-European trade and payments, the ECA proposed in December 1949 the creation of a European payments union. The proposed union aimed to offset the bilateral credits and debits of participating countries, leaving each in net surplus or deficit to the group as a whole. Net balances would then be financed partly with ECA dollars, partly with new credits or gold payments by surplus and deficit countries respectively. The latter provision would introduce an automatic incentive for creditors to correct persistent surpluses by diverting a larger share of their exports to the dollar area and for creditors and debtors alike to adjust their exchange rates and internal fiscal and monetary policies in the interest of European equilibrium. At the same time, the provision for multilateral offsetting would enable countries in net surplus with the group to manage their bilateral deficits without resorting to quantitative import quotas or other restrictions on trade: a deficit with one country could be offset by a surplus with another. Through such provisions, the ECA hoped to reduce the dollar gap, eliminate the barriers to intra-European trade, coordinate national monetary policies, and integrate economies.¹⁹

For policy makers in London, however, merging the British and Continental economies meant shelving their socialist programs, importing Europe's unemployment, and subjecting their economy to greater competition and a potential loss of markets, revenues, and reserves. Such sacrifices, they warned the Americans, would make it difficult to shoulder their share of Europe's defense burden, protect their leadership of the sterling area, and maintain their commitments in other areas of the world that were vital to the security and interests of the Western alliance. For these reasons, they insisted, any payments agreement would have to exempt sterling from multilateral offsettings, leave their bilateral agreements intact, and protect their right to impose quantitative import restrictions on a unilateral and discriminatory basis.²⁰ The British, as the ECA complained, refused to limit "the exercise of national sovereignty, confining its absolute and arbitrary exercise to the legitimate field in which it would not conflict with the economic needs of Western Europe as a whole." They would not submit to the automatic checks on national policy that were inherent in the idea of partial gold settlements. Nor would they abandon the bilateral bargaining and artificial restrictions that prevented readjustment and the "creation of a single, wide, competitive market" in Western Europe.²¹

¹⁹ Hoffman to Harriman, October 25, 1949; Foster to Harriman, November 26, 1949, ECA File, box 8; Hoffman to Harriman, December 3, 1949, *ibid.*, box 51; Harriman to Hoffman, December 3, 9, 1949; Harriman to Hoffman, December 12, 14, 1949, *ibid.*, box 8; and Richard Bissell to Nitze, December 15, 1949, RG 59, Records of the Policy Planning Staff [hereafter, RG 59, PPS Records], box 27, Europe folder, 1949.

²⁰ U.S. Delegation at the Tripartite Preparatory Meetings in London to Acheson, April 25, 26, 28, 1950; U.S. Delegation at the Tripartite Foreign Ministers Meeting in London to Under Secretary of State James Webb, May 9, 1950; Acheson to Webb, May 14, 1950, *FRUS*, 1950, 3 (Washington, 1972), 865–69, 881–83, 886–90, 1018–22, 1061–67. Also see U.S. Department of State, "Essential Elements of US-UK Relations," April 19, 1950, *ibid.*, 869–81. For a summary of the British terms, see Hoffman to Katz, March 11, 1950, ECA File, box 64; ECA, report for the State Department, April 14, 1950, *FRUS*, 1950, 3: 646–52; and ECA, memorandum for the secretary of state, May 5, 1950, Harriman Papers, European Payments Union folder.

²¹ ECA, report for the State Department, April 14, 1950. Also see Hoffman to Harriman, March 31, 1950, ECA File, box 64; and Henry Tasca, memorandum to Katz, March 13, 1950, Harriman Papers, European Payments Union folder.

Under these circumstances some policy makers began discussing a Continental payments union that would exclude Britain. But this idea had “an air of unreality” to most of those in the ECA. The Scandinavians would certainly refuse to participate without the British, and, even if other Marshall Plan countries could reach agreement, they feared that Germany would dominate a strictly Continental group or that “competitive jockeying” over its position would divide the Western system into separate British and Continental blocs, drive a wedge between the United States and Britain, and “mean a setback to Europe and to Atlantic community cooperation.”²² Similar thoughts came from George Kennan’s Policy Planning Staff, from a meeting of American ambassadors in Paris, and from position papers composed in the State Department in the months prior to the London Foreign Ministers Conference of May 1950.²³ Those involved generally agreed that some form of economic union was needed to put Europe on a self-supporting basis and to contain both German and Soviet power. But they agreed as well that the prospects for union did not seem bright so long as the British refused to make the necessary sacrifices. Germany would dominate a Continental union, and fears of this would make it difficult to win continued French support for Germany’s revitalization and reintegration.

The remaining alternative seemed to involve some formula for reconciling British commitments at home and abroad with American plans for an integrated Europe balanced between British and German power. And because the problem was one of financial constraints on British diplomacy, the solution seemed to lie in what one State Department memorandum termed a “‘share-the-wealth’ plan,” under which the British would adopt a more positive approach to the Continent and, in return, “we [would] assume unto ourselves at least the partial obligations of the sterling block [*sic*].” British and American leaders actually hammered out an arrangement like this during the London Foreign Ministers Conference. The British endorsed American economic policy objectives so long as they squared with British responsibilities to the sterling area. The Americans pledged to assist in these responsibilities by supporting pound sterling.²⁴ This understanding then defined the basis for Britain’s participation in a European payments union. Specifically, the

²² Harriman to Hoffman, April 3, 1950, ECA File, box 18. Also see Hoffman to Katz, March 11, 1950, *ibid.*, box 64; and Hoffman to Harriman, March 31, 1950.

²³ Policy Planning Staff, Minutes of the 81st Meeting, May 20, 1949, RG 59, PPS Records, box 32, Minutes of Meetings folder, 1949; Minutes of the 84th, 87th, 99th, 100th, 101st, 102nd Meetings, May 25, 27, June 8, 13 (two meetings), 14 (two meetings), 1949, *ibid.*, box 27, Europe folder, 1949; Summary Record of a Meeting of United States Ambassadors at Paris, October 21–22, 1949, *FRUS*, 1949, 4: 472–96; Minutes of the Seventh Meeting of the Policy Planning Staff, January 24, 1950, *FRUS*, 1950, 3: 617–22; Department of State, “Essential Elements of US-UK Relations”; and Memorandum Prepared in the Bureau of German Affairs, [February 11, 1950], *FRUS*, 1950, 4 (Washington, D.C., 1980), 597–602. Also see Perkins, memorandum to Acheson, January 24, 1950, *FRUS*, 1950, 3: 1610–14.

²⁴ Memorandum Prepared in the Bureau of German Affairs, [February 11, 1950]. Also see Minutes of the Seventh Meeting of the Policy Planning Staff, January 24, 1950; and Department of State, “Essential Elements of US-UK Relations.” For a summary of the American position, see Department of State, “Essential Elements of US-UK Relations.” Ambassador Philip C. Jessup outlined this position for British officials during the Tripartite Preparatory Meetings that preceded the Conference of Foreign Ministers. See U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, April 25, 1950. For the informal Anglo-American agreement, see U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, April 30, 1950; Agreed United States/United Kingdom Report, “Continued Consultation and Co-ordination of Policy,” May 6, 1950, *FRUS*, 1950, 3: 890–92, 1072–74.

British agreed to a multilateral offsetting of sterling balances, while the Americans accepted special terms that safeguarded Britain's reserves and assured sterling's position as a leading reserve and trading currency. They permitted countries that had an overall surplus with the union to accept settlement in sterling, rather than in gold or dollars. They also accepted provisions that gave the British a wider credit margin with the union than had been planned, limited their gold payments to the union, covered part of these payments with ECA dollars, and allowed the British to reimpose quantitative import restrictions on a multilateral basis, if this became necessary to protect their reserves.²⁵

Anglo-American understanding led in July 1950 to OEEC agreement on the principles of the European Payments Union (EPU).²⁶ The agreement included the provisions for multilateral offsetting and partial gold settlement that the ECA had hoped would set the stage for intra-European monetary integration and trade liberalization. Indeed, concurrent with the EPU accord, the participating countries pledged to remove quantitative restrictions on 60 percent of their private imports from each other. They also agreed with the ECA that liberalization and nondiscrimination should go hand in hand. According to a set of trade rules that the OEEC adopted when approving the EPU accord, all existing and future measures of trade liberalization were to be applied equally to imports from other members of the group, as were any restrictions that remained after January 1951.²⁷

As we will see, Anglo-American leaders were unable to forge a similar compromise when it came to giving the EPU's Managing Board positive powers to coordinate national policies. Nevertheless, the EPU agreement and the new trade rules seemed to give real substance to the idea of Western Europe as a single, integrated market. Nor was this the only front on which progress was made. In May 1950, for example, the Western powers agreed to relax further their controls on Germany.²⁸ By the end of June, they had raised industrial production in Marshall Plan countries 24 percent above prewar levels. European exports and intra-European trade had climbed 20 percent and 17 percent respectively. Agricultural production had gone up, the dollar gap had declined, and inflation had come under control. American Marshall planners saw these successes as a validation of their corporative vision of an integrated European economic system similar to the

²⁵ For this part of the story, see Hoffman to Harriman, March 3, 11, 31, 1950, ECA File, box 64; Office of the Special Representative [hereafter, OSR], "Possible Reconciliation Between the EPU System and the Sterling System," April 6, 1950, Treasury Records, Acc. 66A186, box 81, folder EPU/21/300—Original Negotiations and Drafting of EPU Agreement, vol. 1; John Kenney, ECA mission chief, London, to Hoffman, April 13, 1950, ECA File, box 16; Foster to Harriman, two telegrams dated April 17, 1950, RG 286, Acc. 53A177, box 112, Inter-European folder; Hoffman to Harriman, Katz, and Tasca, May 4, 1950, ECA File, box 65; Harriman to Hoffman, May 5, 1950, RG 286, Acc. 53A177, box 18; ECA, memorandum for the secretary of state, May 5, 1950; Kenney to Hoffman, May 14, 1950, ECA File, box 16; and Harriman to Hoffman, May 15, 1950, *FRUS*, 1950, 3: 658–59.

²⁶ For an outline of the terms of the agreement, see Katz to Hoffman, Foster, and Bissell, June 18, 1950, ECA File, box 18; and Harriman to Hoffman, June 20, 1950, *ibid.*, box 20. Also see ECA, *Ninth Report to Congress* (Washington, D.C., 1950), 26–31; and Harriman to Hoffman, July 11, 1950, ECA File, box 20.

²⁷ Foster to Harriman, June 27, 1950; Bissell to Harriman, July 4, 1950, ECA File, box 66; Katz to Hoffman, June 6, 1950; OSR to Hoffman, June 7, 1950, *ibid.*, box 18; and OSR to Hoffman, June 11, 1950, *ibid.*, box 20. Also see Diebold, *Trade and Payments in Western Europe*, 172–75.

²⁸ See "Declaration for the Three Foreign Ministers on Germany," May 22, 1950, *FRUS*, 1950, 3: 1089–91, and 1089n.1.

one that existed in the United States, where “a large domestic market with no internal trade barriers” had supposedly made possible a remarkable record of economic growth and social stability.²⁹ At the same time, moreover, they had supplemented their trade and payments initiatives with simultaneous efforts to build supranational institutions of coordination and control in Europe and new national and transnational networks of public-private cooperation and power sharing. And since these efforts were also part of their plan to boost productivity, integrate economies, and re-create the American brand of neocapitalism in Europe, it seems worthwhile to consider them now in some detail.

ONE OF THE HALLMARKS OF AMERICAN RECOVERY POLICY was the ECA’s effort to forge both national and transnational links between private economic groups and between these groups and government authorities. Through such links it hoped to build a private alliance behind the Marshall Plan, equip Europeans with American production skills, and maximize the prospects on the Continent for transnational action and economic integration, productive abundance and social peace. This effort started in the United States, where the ECA itself became the hub in an elaborate system of public-private cooperation and power sharing. It is therefore important to begin with this system in America and then see how it stretched across the Atlantic.

In the United States, the goals pursued in recovery policy were inextricably tied to the administrative structure that was used. For policy makers in the Truman administration, the need to revitalize industry, maximize output, and liberalize trade, together with the need to appease Congress, required a bipartisan recovery administration that could recruit managerial talent from the private sector and guarantee a business-like efficiency in operational matters. Accordingly, draft legislation submitted to Congress in 1948 called for a new agency, the Economic Cooperation Administration, to handle the operational aspects of the recovery program subject to the State Department’s control in areas relating to foreign policy.³⁰ For Senator Arthur Vandenberg and other members of Congress, however, an efficient and successful recovery program required greater limitations on the State Department’s authority and a larger role for managers from the private sector. The primacy of private management followed logically from their stress on the ERP’s economic objectives in Europe. They were no less interested in relieving suffering and combating communism, yet achieving these goals meant stabilizing currencies, fixing realistic exchange rates, reviving industry, liberalizing trade, and, through these and other reforms, fostering integration and boosting productivity. Steering this course, they agreed, required expert management by men with a practical knowledge of the complicated agricultural, industrial, financial, and labor problems involved. But it also required an administrative structure

²⁹ House, *Foreign Assistance Act of 1948*, 254; and ECA, *Ninth Report to Congress*, 3–7.

³⁰ The administrative recommendations in this legislation were the result of intense bureaucratic bargaining inside the Truman administration. See Hadley Arkes, *Bureaucracy, the Marshall Plan, and the National Interest* (Princeton, 1972), 59–83.

that would permit these men to apply their knowledge without political interference from the State Department. It required, in other words, a truly independent agency managed by private leaders who controlled operational decisions and shared with public officials the responsibility for making policy.³¹

Those involved in the congressional hearings could cite numerous precedents for this kind of public-private power sharing. Some noted the wartime control arrangements worked out by public and private leaders under the aegis of the War Production Board. Others pointed to the President's Committee on Foreign Aid, a special fact-finding group of university experts and representatives of labor, industry, and agriculture that President Truman had established to investigate the impact of a major aid program on the American economy. Still others cited the Commerce Department's success with its Business Advisory Council (BAC) and voluntary agreements with industry. Secretary of the Interior J. A. Krug made the same point about his department's collaboration with such groups as the National Petroleum Council. "Government-industry action" and "maximum" cooperation, he told the Senate Foreign Relations Committee, would satisfy both European and American requirements without "centralizing authority" in Washington.³²

Witnesses suggested a variety of proposals for achieving this kind of public-private cooperation in managing the recovery program. From business and farm leaders came calls for a policy board composed of both government officials and representatives of private economic groups, especially organized industry, labor, and agriculture. From labor leaders came proposals under which public policy makers would be guided by the views of private advisory committees. And from a variety of other witnesses came schemes for a blend of direct and indirect representation, to be achieved by staffing the ECA with representatives from private groups, establishing private advisory committees, and preserving some role in the recovery program for those government agencies, particularly the Commerce and Agriculture departments, that were closely tied to private interests through a network of cooperating committees.³³

One organization embracing the latter idea was the Brookings Institution. Its report, solicited by Senator Vandenberg, acknowledged the "special character of the task" involved in rebuilding Europe, a task entailing "economic and business" responsibilities that the State Department could not assume. Needed was a "new and separate agency," headed by a single administrator of cabinet rank who would have direct access to the president and "primary responsibility for the formulation of operating policies and programs." This agency should be exempt from various

³¹ U.S. Congress, House of Representatives, Committee on Foreign Affairs, *Hearings, United States Foreign Policy For a Post-War Recovery Program*, 80th Congress, 1st and 2d sess., 1947-48 (Washington, D.C., 1948), 247, 257, 810-11 [hereafter, House, *Recovery Program Hearings*]; and U.S. Congress, Senate, Committee on Foreign Relations, *Hearings, European Recovery Program*, 80th Congress, 2d sess., 1948 (Washington, D.C., 1948), 751, 806, 808 [hereafter, Senate, *ERP Hearings*].

³² Senate, *ERP Hearings*, 366, 279, 359-60, 365, 851, 1394; and House, *Recovery Program Hearings*, 569, 1445. Even Secretary of State Marshall had noted the successful work performed "by a number of government agencies in cooperation with business, agriculture, and labor groups"; Arkes, *Bureaucracy and the Marshall Plan*, 67.

³³ Senate, *ERP Hearings*, 728, 734-35, 807-08, 835, 851, 1039, 1116-17, 1127, 1293, 1346-47; and House, *Recovery Program Hearings*, 582, 590, 594, 810-11, 941-42, 1311-12, 1386-87, 1413, 1445-46.

federal regulations, particularly those limiting salaries, in order to attract managers from the private sector, and it should have a public advisory board and private advisory committees to maintain regular consultation between its administrator and representatives of "industry, labor, agriculture, and . . . other private citizens." Because the ERP was not "a purely business job," the new agency must not encroach on roles properly played by government departments. The Commerce and Agriculture departments were to retain their authority over export allocations, the State Department was to negotiate bilateral aid agreements with the participating countries in Europe, and the National Advisory Council was to determine specific financial policies. These were often "political" as opposed to "business" responsibilities, and, although it was assumed that the two could be harmonized through consultation and cooperation, each had its own institutional requirements.³⁴

These ideas were incorporated into the Foreign Assistance Act of 1948.³⁵ Included as well were provisions insuring the bipartisan nature of the Public Advisory Board, allowing the ECA to establish its own missions abroad, and making both its mission chiefs and its special representative in Europe directly responsible to the administrator rather than to the secretary of state. Through such provisions, Vandenberg and others hoped to insure the ECA's autonomy and substitute for the State Department's "political" control a bipartisan public-private partnership in which essentially private leaders made operational decisions, exerted influence through a network of advisory committees, and collaborated with their public counterparts in the formulation of policy. The administrator, to be sure, would be a "public" official appointed by the president with the consent of Congress. But he was to be recruited from the private sector, function like a private executive, and run his agency like a "business enterprise."³⁶ Because the job required a man with "particularly persuasive economic credentials unrelated to diplomacy," it was the "overriding Congressional desire," Vandenberg told Marshall in vetoing the appointments of William Clayton and Dean Acheson to this position, "that the ERP Administrator come from the outside business world . . . and *not* via the State Department." In the end, Vandenberg himself selected the administrator, choosing Paul G. Hoffman, president of Studebaker Corporation and one of the industry representatives on the President's Committee on Foreign Aid.³⁷

The result was an administrative structure that deliberately dissolved the distinction between public and private spheres, and did so as part of a strategy for achieving the goals of economic integration and greater productivity. Under the Foreign Assistance Act, for example, the ECA had the Public Advisory Board, to which Hoffman appointed representatives of organized business, labor, and

³⁴ For the conclusions of the Brookings report, see Senate, *ERP Hearings*, 855–60. On Vandenberg's solicitation of the report, see *ibid.*, 74.

³⁵ See House, *Foreign Assistance Act of 1948*.

³⁶ Vandenberg to Carl M. Saunders, January 2, 1948, Bentley Historical Library, University of Michigan, Ann Arbor, Arthur H. Vandenberg Papers [hereafter, Vandenberg Papers], box 3, Correspondence folder, January 1948. Also see Vandenberg, as quoted in Arkes, *Bureaucracy and the Marshall Plan*, 84.

³⁷ Vandenberg to Marshall, March 24, 1948, HSTL, Harry S. Truman Papers, President's Secretary's File, Subject File: George C. Marshall. Also see Arkes, *Bureaucracy and the Marshall Plan*, 100.

agriculture. It also had private advisory committees to assist the administrator with specific problems, including the Oil Price Committee, Advisory Committee on Fiscal and Monetary Problems, Advisory Committee on Overseas Development with a subsidiary investment panel, and Advisory Committee on Reparations. In addition, Hoffman cooperated with the Council on Foreign Relations, commissioning that group to report on United States goals in Western Europe and the means for achieving them. He also worked closely with the President's Advisory Committee on Financing Foreign Trade, and he had the "benefit of advice informally given by men such as Russell Leffingwell and Bernard Baruch."

The advisory committees were dominated by prominent figures from the world of business and finance. The Committee on Financing Foreign Trade was headed by Winthrop Aldrich, board chairman of the Chase National Bank. William L. Clayton led the ECA's investment panel on overseas development. The Committee on Fiscal and Monetary Problems included George Harrison of the New York Life Insurance Company, Edward Brown of the First National Bank of New York, Walter Steward of the Rockefeller Foundation, and Joseph Dodge of Detroit Bank. The Advisory Committee on Reparations was headed by George Humphrey of the Hanna Company and included such "top-flight" industrialists as Charles Wilson of General Motors, Gwilyn Price of Westinghouse, John McCaffrey of International Harvester, and Frederick Geier of the Cincinnati Milling Machine Company.³⁸

Similar men, "Wall Street wolves" according to Hoffman's critics, dominated the top positions in the ECA during its first year of operation. A number of important slots were occupied by professionals and career public servants.³⁹ But more typical were men with corporate backgrounds similar to Hoffman's. W. Averell Harriman, the special representative in Europe, was a senior partner in the Wall Street firm of Brown Brothers, Harriman. Howard Bruce, the deputy administrator in Washing-

³⁸ Hoffman to Clarence Francis, October 1, 1948, HSTL, Paul G. Hoffman Papers [hereafter, Hoffman Papers], box 21, ECA Correspondence folder. Also see Richard Heindel to Vandenberg, August 24, 1948, Vandenberg Papers, box 3, Correspondence folder, August 1948; Hoffman to C. A. MacDonald, August 23, 1948, Hoffman Papers, box 1, Chronological File folder; Hoffman to American Embassy, Paris, October 15, 1948, ECA File, box 45; Council on Foreign Relations, "Studies on Aid to Europe," November 30, 1948, HSTL, William L. Clayton Papers, box 73, Council on Foreign Relations folder. For information on the Aldrich committee and the ECA's investment panel, see RG 286, Acc. 53A405, box 50, Finance-Investments (Aldrich committee) folder.

³⁹ Hoffman to Frank Gannett, July 15, 1948, Hoffman Papers, box 1, Chronological File folder. By "top positions" I mean the administrator and the special representative in Europe, their deputies and assistants, the general counsel and comptroller, the ECA's mission chiefs abroad, and the directors of its key divisions as identified in the text. The directors of such divisions as administration and administrative services, statistics and reports, China program, organization and management, personnel, and security have not been considered. Information on the directors of the divisions of operations, strategic materials, procurement operation, and program coordination has not been located. The information that follows in the text is based on the roster of personnel listed in the second edition of the ECA's pamphlet, "American Business and European Recovery" (Washington, D.C., 1948). Except where noted in the text, biographical information on the men listed on this roster comes from *Who's Who in America*, *Who Was Who in America*, *Current Biography: Who's News and Why*, *The National Cyclopaedia of American Biography*, and *Biographical Dictionary of American Labor Leaders*. Arthur Smithies, director of the Fiscal and Trade Policy Division in Washington, and Arthur Marget, who held a similar position at the ECA's headquarters in Paris, were professional economists with considerable government experience. Dennis Fitzgerald and Ben Thibodeaux, who headed the Food and Agriculture divisions in Washington and Paris, were agricultural economists. Milton Katz, a professor at the Harvard Law School, and Richard M. Bissell, Jr., an MIT economist, served respectively as general counsel in Paris and assistant deputy administrator in Washington.

ton, was director and board member of several large business firms. William Foster, Harriman's deputy in Paris, was president of a steel products company. Among the chief assistants to the administrator and deputy administrator in Washington were C. Tyler Wood, formerly a partner in the law firm of Gilbert, Elliot, and Company; Wayne C. Taylor, a Chicago banker before entering government service in the 1930s; James Cleary, a vice president in a leading advertising agency; Samuel Richards, an executive with the Studebaker Corporation; and Maurice Moore, a partner in the law firm of Cravath, Swaine, and Moore. Men with similar experience held the positions of comptroller, budget director, general counsel, and director of information.⁴⁰ Business representation was particularly strong in the ECA's industry divisions. The one in Washington was headed by Samuel Anderson, a partner in several large investment companies. In Paris, George W. Perkins of Merck and Company served in a similar capacity. And under Perkins were Clarence Randall, a vice president of Inland Steel Company; Cecil Burrill, an executive with Standard Oil of New Jersey; George Green, formerly a vice president of General Motors; Walter Cisler, a vice president of Detroit-Edison; and Godfrey Rockefeller, "a director of quite a number of corporations."⁴¹

Prominent business leaders also headed most of the ECA's overseas missions in 1948: in the United Kingdom, Thomas K. Finletter of Coudert Brothers; in Italy, James Zellerbach, board chairman of Crown Zellerbach Corporation; in France, David K. E. Bruce, a Baltimore businessman; and in Belgium, James G. Blaine, president of the Midland Trust Company of New York. In addition, the mission chiefs in Norway, Denmark, Turkey, Austria, Greece, and Sweden were, respectively, August Staley, an Illinois banker; Charles Marshall and Russell Door, both prominent New York attorneys; Westmore Wilcox, a partner in various New York investment firms; John Nuveen, a Chicago businessman; and John Haskell, a financial executive.⁴²

Noteworthy, too, was the previous government service of most of these business leaders.⁴³ Many belonged to one or more of those prestigious private associations, especially the BAC and the Committee for Economic Development (CED), that routinely advised public officials and served as recruiting pools for government positions. Hoffman had helped found the CED, and both he and Harriman were

⁴⁰ These were Eric Kohler, an accounting executive; Norman Taber, an investment and financial consultant; Alexander Henderson, a partner in the law firm of Cravath, Swaine, and Moore; and Bryan Houston, a vice president of the Pepsi-Cola Company.

⁴¹ Perkins actually succeeded Langbourne Williams, who served briefly as division head in 1948. Harriman probably recruited Williams from the Business Advisory Council. For information on members of the Industry Division in Paris, see Summary of Conference of Hoffman, Harriman, Members of ECA—Paris Staff, and Country Mission Representatives, Paris, July 22–23, 1948, RG 286, Acc. 53A405, box 1, Paris Conference folder, July 22–28.

⁴² The remaining four mission chiefs listed in the ECA's pamphlet, "American Business and European Recovery," were Harriman, who officially headed the ECA mission in bizonal Germany; Joseph Carrigan, a former dean of the Agriculture School of the University of Vermont, who was chief of mission in Ireland; Alan Valentine, president of the University of Rochester and a director of various business firms, who headed the ECA mission in the Netherlands; and Roger Lapham, board chairman of the American Hawaiian Steamship Company and a member of the Business Advisory Council, who led the ECA mission in China.

⁴³ It should also be noted that virtually all top positions in the ECA were held by college graduates, ten of them by Harvard graduates and over half of them by graduates of such other prestigious schools as Yale, Princeton, Brown, MIT, Swarthmore, Johns Hopkins, and the University of Pennsylvania.

leading figures in the BAC. Both could agree that “top people from every industry” were needed to manage the recovery program efficiently and to solve the difficult economic and technical problems involved in stabilizing finances, liberalizing trade, and boosting productivity. Harriman recalled turning to the BAC when staffing his Paris office, and he thought it likely that Hoffman had used the CED for similar purposes.⁴⁴

Along with business leaders, Hoffman tried to incorporate other economic groups into schemes of corporatist collaboration with the government. The major farm groups were represented on the ECA’s Public Advisory Board, cooperated with its overseas missions, and worked closely with its food and agriculture divisions. Organized labor also played an important part, not only in countering communist attacks on the Marshall Plan but in persuading European workers to work harder, defer consumption for the sake of investment, accept temporary unemployment, and make the other sacrifices necessary to raise production and achieve “effective European collaboration and economic integration.” In Europe, it was argued, “deep-seated union policies and worker habits” would have to give way to the “[p]roductivity stress” typical of America. European labor leaders had to recognize the need to adjust wage and price differentials between countries, reduce the barriers to labor migration, and replace the old system of national “self-sufficiency with more efficient production through specialized industries” operating in a European-wide market. And to expedite this process, union officials in the United States had to start sharing with “European labor and management” some of their ideas on the best way to organize industry and increase productivity.⁴⁵

Nor were American trade unions reluctant to assume such a role. They endorsed the ERP in public statements that emphasized the importance of “increased production” and “economic integration” in Western Europe. They also urged labor’s participation in a bureaucratic management of the recovery program and cooperated with public officials in shaping the international aspects of American labor policy.⁴⁶ The Department of Labor, for example, had already organized an Office of International Labor Affairs under Philip Kaiser and a Trade Union Advisory Committee that was collaborating with Kaiser, and with officials of the War and State departments, in shaping overseas labor policy. The State Department had also created the new post of labor attaché (choosing for these positions men close to American trade unions),⁴⁷ and its Division of International Labor and

⁴⁴ Summary of Conference, Paris, July 22–23, 1948; and interview with W. Averell Harriman, January 6, 1982.

⁴⁵ Val Lorwin, “Labor Participation in the Organization for European Economic Cooperation,” April 30, 1948, RG 59, file 840.5043/5–1448; Under Secretary of State Robert A. Lovett to American Embassy, Paris, December 8, 1948, RG 59, file 840.50 Recovery/11–2448; and Nitze, “Labor’s Role in the European Recovery Program,” August 15, 1948, copy in RG 59, PPS Records, box 50, Paul Nitze (Speeches & Articles, 1945–1953) folder.

⁴⁶ See, for example, the AFL’s declaration on foreign policy, as quoted in Howard Bruce to Harriman, November 24, 1948, ECA File, box 45; and AFL memorandum to President Truman, December 19, 1947, HSTL, Truman Papers, President’s Official File, folder 426-L (March 1951–1953).

⁴⁷ Peter Weiler, “The United States, International Labor, and the Cold War: The Breakup of the World Federation of Trade Unions,” *Diplomatic History*, 5 (1981): 1–22. Also see Kaiser to Erick Kocher, American Embassy, Brussels, November 6, 1947, HSTL, Philip Kaiser Papers, box 2, Labor Attachés and State Department Officials—General Correspondence folder.

Social Affairs was acting as liaison between the trade unions and the secretary's office. At the same time, Marshall and others in the State Department were considering qualified labor leaders for ambassadorial positions when they became available and were supporting representation for organized labor on the American delegations to various international conferences and on such UN agencies as the Economic and Social Council and the World Health Organization. In addition, they were collaborating with American trade union leaders to combat communism in the European labor movement, break up the communist-led World Federation of Trade Unions, and organize the European Recovery Program—Trade Union Advisory Committee to mobilize European workers behind the Marshall Plan and the noncommunist labor international that was formed in 1949.⁴⁸

A substantial foundation existed, therefore, on which those seeking to integrate labor into Marshall Plan initiatives could build. Kaiser represented labor's views on the interdepartmental steering committee that studied the recovery program. Officials of both the American Federation of Labor (AFL) and the Congress of Industrial Organizations (CIO) served on the President's Committee on Foreign Aid.⁴⁹ Secretary Marshall asked the AFL for a list of possible labor appointees to the ECA, an agency expected to have "quite an immediate tie-in" with the labor, farm, and industry groups that supplied commodities, services, and personnel to the administrator. The list and union participation led to actual appointments. Arlon Lyon of the Railway Labor Executives' Association, George Meany of the AFL, and James Carey of the CIO were appointed to the ECA's Public Advisory Board. Bert Jewell of the AFL and Clinton Golden of the CIO became Hoffman's chief labor advisors in Washington. Boris Shishkin of the AFL headed the Labor and Manpower Division at Harriman's headquarters in Paris.⁵⁰ In addition, trade union officials were eventually appointed as mission chiefs in Norway and Sweden and as labor advisors to missions in other participating countries. Their task was to develop ties between American and European labor organizations and to help formulate policy on various "economic, social, technical and other problems affecting the European workers and their trade unions." "The trade unions of America," as Hoffman explained in 1949, had "a status of full partnership in the ECA not only from the standpoint of operations but from the standpoint of making policy."⁵¹

⁴⁸ Lovett to William Green, AFL, April 14, 26, June 17, 1948, RG 59, file 840.50 Recovery/6-1548; Cleon Swayzee, director of the Division of International Labor and Social Affairs, memorandum, March 9, 1949, HSTL, Dean G. Acheson Papers, box 64, Memoranda of Conversations folder; Swayzee, memorandum to Acheson, March 8, 1949; memorandum of conversation, March 14, 1949; Matthew Woll, AFL, to Acheson, March 14, 1949; Acheson to Woll, April 4, 1949, RG 59, file 800.5043/3-1449; and Weiler, "The United States, International Labor, and the Cold War."

⁴⁹ Kaiser to Kocher, November 6, 1947, and "Attitude of Labor towards the ERP," February 2, 1948, RG 59, file 800.5043/2-248.

⁵⁰ Memorandum of conversation between State Department and AFL officials, April 6, 1948, RG 59, file 840.50 Recovery/4-648; Lovett to Green, April 26, 1948, and memorandum to Harriman, April 26, 1948, *ibid.*, Recovery/4-2048; and *New York Times*, June 3, 1948, p. 14. Also see the ECA's pamphlet, "American Business and European Recovery"; and Hoffman to American Embassy, Paris, July 2, 1948, ECA File, box 44.

⁵¹ There are two letters from Hoffman to the American Embassy, Paris, dated July 2, 1948; both are noteworthy. See ECA File, box 44. Also see Harriman to Hoffman, July 8, 1948, *ibid.*, box 5; Hoffman, "Weekly Report of the Administrator," November 8, 1948, Hoffman Papers, box 22, ECA, Miscellaneous folder, 1948-1949; Harriman to Hoffman, February 23, 1949; Hoffman to Harriman, March 2, 1949, RG 286, Acc. 53A405, box 1, OSR folder, 1949; Hoffman to Harriman, April 9, 1949, ECA File, box 47; and Hoffman to Harriman, February 8, 1949, ECA File, box 46.

Nor were the patterns of partnership limited to the ECA and private economic groups in the United States. During the same period, Hoffman and others were also trying to link their system of power sharing to a similar system in Europe. The results amounted to a transnational network of public-private cooperation, one that reflected the ECA's faith in the capacity of managerial approaches and corporatist collaboration to achieve efficiency, minimize disruptive social competition, and mobilize private interests behind the interrelated goals of economic integration and greater productivity. In many of the participating countries public and private leaders were already cooperating to formulate social welfare and economic policies. But the ECA now wanted organized private groups to be tied to the OEEC and represented on the ERP consultative committees being established by some European governments. In France, for example, the ECA supported a government decision to establish a labor advisory committee that would meet regularly with the ministries responsible for the recovery program. In Italy, it brought noncommunist trade unions together in a similar body.⁵² It also persuaded European leaders to permit regular meetings between the OEEC's technical groups and the Trade Union Advisory Committee on matters relating to the labor aspects of the recovery program. And, together with the OEEC, it established liaisons with the International Federation of Agricultural Producers, a group that would eventually function as the OEEC's advisory body on agricultural policies in Europe.⁵³

Business and banking groups made up another part of this transnational network. With support from the ECA and State Department, the Aldrich committee established three American investment groups to cooperate with similar, government-recognized groups in Belgium, Britain, and France. Their job was to identify potential development projects for American investors.⁵⁴ The ECA also played a part in organizing American corporate groups to collaborate with the British Dollar-Exports Board, established jointly by the Associated British Chambers of Commerce, the Federation of British Industries, and the British Trades Union Congress. Other participating countries soon created their own dollar boards, which, together with their American partners, sought ways to increase European exports to the Western Hemisphere.⁵⁵ A similar pattern took shape around the Anglo-American Council on Productivity. Proposed by Hoffman and British Chancellor of the Exchequer Sir Stafford Cripps in July 1948, the council brought together British and American trade union and management leaders to investigate capital plant and bottleneck problems in Britain and determine how

⁵² Harriman to Hoffman, October 27, 1949, ECA File, box 8; and W. K. Knight, memorandum of conversation, October 1, 1948, RG 59, file 840.50 Recovery/10-148.

⁵³ Foster to Hoffman, March 26, 1949; Caffery to Hoffman from Clinton Golden and Bert Jewell, April 13, 1949; Harriman to Hoffman, May 21, 1949, ECA File, box 6; Harriman to Hoffman, May 29, June 25, July 6, 1949, *ibid.*, box 7; and Harriman to Hoffman, October 28, 1949, *ibid.*, box 8.

⁵⁴ Harland Cleveland, memorandum to Foster and Bissell, November 10, 1949, HSTL, John D. Sumner Papers, box 8, Economic Cooperation Administration, General ERP and Marshall Plan folder; and Winthrop Aldrich, "Report on the Establishment of the Committees of Banking Institutions in France and Belgium to Facilitate the Implementation of the Point IV Program," attached to Cleveland memorandum to Foster, November 22, 1949; Cleveland to Barry Bingham, December 7, 1949, RG 286, Acc. 53A405, box 50, Finance-Investments (Aldrich committee) folder.

⁵⁵ See RG 286, Acc. 53A405, box 1, British Dollar Exports Board folder; and Bingham to Hoffman, October 22, 1949, Treasury Records, Acc. 67A1804, box 4, France, Aid Program folder, vol. 2.

American labor and management could assist the British in increasing productivity. The British group was nominated by the Federation of British Industries and the British Trades Union Congress, acting through the National Production Advisory Council established during the war. The American group was chosen by Hoffman, the industry representatives in consultation with Philip Reed of General Electric, who served as chairman, and the labor members in consultation with the ECA's labor advisors and the leading American trade unions.⁵⁶

The council held its first meeting in London at the end of 1948, where it established committees for the study of industry organization, capital investment trends, and production bottlenecks. The American members then toured several British plants, where they found the biggest barrier to increased production to be the frequently "complacent attitude" of British labor and management toward scientific production procedures, technical efficiencies, and restrictive business practices. This they hoped to change. And while the British ruled out action in regard to restrictive practices, the council did agree on a program of "industrial education" at all levels of British industry, the creation of agencies to spread American "know-how," and arrangements under which teams of British managers, technicians, and shop foremen could study production methods in the United States.⁵⁷ By the time the council disbanded in 1952, it had sponsored visits to the United States by sixty-six productivity teams, distributed over five hundred thousand copies of their reports, and published major studies on standardization and simplification in industry.⁵⁸

Still another example of transnational action and power sharing in the service of productivity was that connected with the technical assistance program launched by the ECA at the end of 1948. The goal in this case was to stimulate greater efficiency and higher productivity in European industry. To achieve this, the ECA disseminated American technical and scientific information in Europe, conducted "in-plant and academic training" programs for European engineers and plant foremen, and held management seminars, or "clinics," for European executives. It also used engineers and technical consultants as roving ambassadors of American science and selected teams of European technicians, labor leaders, and managers to study industrial organization, production techniques, and labor-management relations in the United States. In organizing the program, ECA officials worked closely with European industrial, labor, and governmental leaders. And on the American side, they cooperated with the National Association of Manufacturers, Chamber of Commerce, National Management Council, and leading labor unions, farm

⁵⁶ Douglas to Marshall, July 30, 1948, RG 59, file 103.ECA/7-3048; Douglas to Marshall, July 31, 1948, ECA File, box 4; Hoffman to American Embassy, London, August 4, 1948, RG 84, box 1035, file 850, Marshall Plan; Harriman to Hoffman, August 5, 1948, ECA File, box 5; Finletter to Marshall, August 5, 1948, ECA File, box 4; Hoffman to Finletter, August 27, 1948, RG 84, box 1035, file 850, Marshall Plan; Finletter to Hoffman, September 3, 1948, ECA File, box 4; Hoffman to Finletter, September 10, 1948, RG 84, box 1035, file 850, Marshall Plan; and Hoffman to Harriman, September 20, 1948, ECA File, box 45. Also see Paul G. Hoffman, *Peace Can Be Won* (Garden City, N.Y., 1951), 101-02.

⁵⁷ Finletter to Hoffman, November 1, 1948, ECA File, box 4; and Anglo-American Council on Productivity, "Report of the First Session" (November 1948), Hoffman Papers, box 24, Economic Cooperation Administration, Pamphlets folder.

⁵⁸ Anglo-American Council on Productivity, "Final Report of the Council" (September 1952), Hoffman Papers, box 24, Economic Cooperation Administration, Publications folder.

groups, and trade associations. Between 1948 and 1951, the ECA spent nearly \$20 million to finance 625 technical assistance projects, maintain approximately three hundred American productivity “experts” abroad, and sponsor several thousand European labor and management visitors to the United States.⁵⁹

Pains were taken to impress these visitors with the close ties between government and labor in the United States, and with the gains in productivity, living standards, and labor peace to be derived from cooperative labor-management relations. One delegation of key Italian labor leaders spent several days touring the plant facilities and talking to the workers of the Crown Zellerbach Corporation, a large paper manufacturer that had recently been selected by the National Planning Association as an outstanding example of labor-management harmony in the United States.⁶⁰ A group of British steel founders heard American labor officials lecture on how “cooperation between management and labor” had resulted in greater productivity and “rising standards of living” for workers in the United States.⁶¹ Still other groups toured farm and industry facilities, learned about the cooperative links between the American government and private economic groups, and received instruction in American labor-training techniques, American methods for arbitrating labor-management disputes, and what Hoffman called the American “miracle of mass production.” “American know-how, wage earners’ freedom, and property available to all” were the blessings that the ECA promised to “bring to pass in Europe,” and these would come, the agency believed, once the Europeans began adopting American ideas for “worker and employer teamwork,” for “more efficient production,” and for eliminating “waste motions and turning out products in [the] manner that American management and labor have worked out.”⁶²

The ECA thought its technical assistance program and the comparable work of the Anglo-American Productivity Council were helping to produce a new European outlook—including a “new spirit of cooperation between labor and management”—by pointing to the impressive gains that American industry had made through intensive competition in a large, internal market, strict methods of product simplification, standardization, and cost accounting, and close collaboration with organized labor, which, “despite occasional differences with management over wages,” generally agreed “that a high standard of living must be supported by a continuously increasing rate of output.”⁶³ The ECA urged other participating countries to emulate the British and establish their own productivity councils. These, too, should be led by labor, management, and government representatives and should sponsor productivity teams, disseminate technical information, and serve “as the central coordinating and directing force in increasing productivity.” By the end of 1951, Austria, Belgium, Denmark, France, the Netherlands, and West Germany had followed the British lead. They had established national

⁵⁹ ECA, *Eleventh Report to Congress* (Washington, D.C., 1950), 49–51. Also see Hoffman to ECA, November 12, 1947; Foster to Hoffman, November 26, 1948; Foster to Hoffman, December 3, 1948, ECA File, box 5.

⁶⁰ Hoffman to Harriman, April 1, 1949, ECA File, box 47.

⁶¹ Hoffman to Harriman, March 15, 1949, *ibid.*, box 46. Also see Hoffman to Harriman, May 19, 1949, *ibid.*, box 48.

⁶² Hoffman to Harriman, February 14, March 10, 1949, *ibid.*, box 46. Also see Hoffman to Harriman, January 7, February 12, 16, 17, 1949, *ibid.*

⁶³ ECA, *Tenth Report to Congress* (Washington, D.C., 1950), 48, and *Ninth Report to Congress*, 65.

"productivity centers" and, through the OEEC, had launched an intra-European technical assistance program under which participating countries began exchanging information and visits between leaders of labor, management, and government.⁶⁴

The European productivity councils and the OEEC's technical assistance programs became additional parts of the public-private alliance through which the ECA worked to export American skill, win support for the Marshall Plan, and replace old patterns of national rivalry and class conflict with a new order of economic integration and corporatist collaboration. To be sure, not all forms of economic integration and public-private cooperation were acceptable. Hoffman and others objected to government-sanctioned arrangements under which private leaders would negotiate economic specialization along national lines. Such arrangements, they worried, might lead to new cartels and other restrictive agreements that would frustrate the rule of comparative advantage, prop up prices, and discourage rational investment, efficient use of resources, and maximum productivity. There was no reason to believe, they concluded, that such agreements could yield "results approximating those attainable by competitive forces" and the normal incentives of a free market. Indeed, some in the ECA talked about using the technical assistance program to acquaint European labor and management teams with the antitrust activities of the Department of Justice and the Federal Trade Commission.⁶⁵

Yet if cartel-type agreements were out of the question, so was an approach that relied wholly on market incentives. The ECA was not seeking "to impose a Hazlitt libertarianism" on Europe or to follow what Hoffman dismissed as a "policy of government non-intervention." He and others also believed in "administrative coordination" through central institutions with the power to harmonize national policies across the ERP area.⁶⁶ This was the third line of American recovery policy, and, like the ECA's attempts to liberalize trade and payments and forge new systems of national and transnational collaboration, it aimed to integrate markets, maximize production, and put the Continent on a self-supporting basis.

THE ORGANIZATIONAL DIMENSION OF THE RECOVERY PROGRAM in Europe, like that in the United States, reflected the intrinsic link between administrative strategy and corporative objectives of American policy. The payments plans of 1948 and 1949 and the European Payments Union of 1950 were organizational mechanisms that the Americans hoped would weld the Continent into an economic unit. They had similar hopes for the EPU's Managing Board and for the Organization for European Economic Cooperation. But in both cases the results fell short of

⁶⁴ ECA, *Seventh Report to Congress* (Washington, D.C., 1949), 53. Also see ECA, *Ninth Report to Congress*, 53, *Tenth Report to Congress*, 66, *Twelfth Report to Congress* (Washington, D.C., 1951), 49; and Bonsal to Hoffman, December 29, 1949, ECA File, box 8.

⁶⁵ Hoffman to Harriman, January 3, 1950, ECA File, box 63. The ECA's views were in response to the government-sanctioned arrangements being proposed by the French. See Katz to Hoffman, January 6, 1950; Harriman to Hoffman, January 10, 1950, *ibid.*, box 17; Hoffman to Harriman, January 11, 1950, *ibid.*, box 63; Katz to Hoffman, February 18, 1950, *ibid.*, box 17; and Hoffman to Harriman, March 31, 1950, *ibid.*, box 64.

⁶⁶ Hoffman to Harriman, January 12, 1950, *ibid.*, box 63. The reference is to Henry Hazlitt, the conservative economist.

expectations, in large part because of Britain's antipathy to central institutions that might compromise its sovereignty, socialist programs at home, and commitments to the Commonwealth and the sterling area. The diplomacy of organization thus concealed a far more profound struggle over the nature of the European and world systems and the British role in both.

The ECA's plan for the European Payments Union had combined market incentives and administrative mechanisms to induce debtors and creditors to move toward equilibrium in overall intra-European payments. As we have seen, the former involved an obligation for creditors and debtors to cover a portion of their surpluses and deficits through new credits or gold payments to the EPU. The administrative mechanism was to consist of continuous consultation among member states, mutual review of their fiscal and monetary policies, and collective recommendations for maintaining equilibrium by adjusting exchange rates or changing national economic policies. Consultation would be institutionalized in the EPU's Managing Board, which was to launch investigations, take action by majority rather than unanimous vote, and have positive powers to coordinate national policies. The board, to be sure, would be less than a supranational authority. But neither would it be another of those "OEEC-type committees" in which national bargaining and government vetoes made decisive action impossible. For policy makers in the ECA, the board represented a compromise between these two organizational strategies, one that would circumvent the question of sovereignty while limiting the exercise of sovereign power through what amounted to national vetoes. On the board, moreover, would be an ECA representative, and backing it up would be the ECA's control over part of the funds that supported intra-European clearings. These arrangements, it was thought, could make the Managing Board an effective instrument of transnational action and a stepping stone toward a truly supranational organization.⁶⁷

Yet the ECA's plan went further than others were willing to go. The agreement on the principles of a payments union that the OEEC reached in July 1950 established the Managing Board of seven members with the right to make decisions by majority vote. But the board's powers were vaguely defined and its decisions subject to review by the OEEC, which could recommend changes in national policy only by the unanimous vote of its members. Nor did the ECA have voting membership on the Managing Board. These limitations were partly the result of opposition in the Treasury Department and the National Advisory Council to a strong European agency that could challenge the jurisdiction of the International Monetary Fund in matters relating to exchange rates or the internal fiscal and monetary policies of its members. But they were also the result of opposition from some OEEC delegations, particularly the British delegation, to a truly independent board with considerable power to dictate national policies. Whatever the source, the results were the same. Although Hoffman and others still thought that the board might develop into a strong coordinating agency, the EPU had to rely largely on

⁶⁷ In addition to the sources cited in note 19, see Hoffman to Harriman, October 25, 1949; Foster to Harriman, November 26, 1949, ECA File, box 50; Harriman to Hoffman, December 3, 1949, *ibid.*, box 8; and Hoffman to Harriman, December 5, 1949, *ibid.*, box 51.

market incentives, not administrative controls, to adjust national policies in the interest of European equilibrium and integration.⁶⁸

Nor was the ECA more successful in shaping the OEEC into an independent agency of economic action and integration. In the months following Marshall's speech at Harvard in June 1947, he and others urged European countries to create a continuing organization with considerable power to transcend sovereignties, set national production targets, allocate scarce resources, and coordinate national investment decisions. By March 1948, when the Committee on European Economic Cooperation met in Paris to form a continuing organization, they were calling for a "major instrument" of economic "cooperation and further integration," one with an "active secretariat" under the leadership of an "outstanding man," national representatives "of standing," and real executive responsibilities.⁶⁹ But the Europeans, particularly the British, were skeptical of an organizational strategy that subjected their trade, investment, and production policies to supranational control. The CEEC's preliminary report of September 1947 called for a joint organization with none of the powers that American leaders envisioned, and the OEEC's charter of April 1948 offered little more than an organizational medium for traditional forms of intergovernmental cooperation. It provided for a secretariat of international character, headed by a secretary general, but with merely routine duties. Administrative responsibility would reside in an executive committee and ultimate authority in a council of national representatives acting through mutual agreement. The new organization could "promote," "investigate," "consider," and "recommend," but it could not act independently of home governments.⁷⁰

Despite these setbacks, American leaders were determined to make the OEEC the "focal point around which closer Western European economic cohesion can be built." They decided to keep bilateral negotiations to a minimum, deal collectively with Europeans through their permanent organization, and saddle the OEEC with as many responsibilities as possible, including devising coordinated annual recovery programs, liberalizing trade, and hammering out the intra-European payments agreements.⁷¹ They also urged participating countries to appoint their "best brains" to the executive committee and the council, convene these bodies at the ministerial level as often as possible, and empower the secretariat to launch investigations and make recommendations. Most important, they wanted the OEEC to create a new

⁶⁸ In addition to the sources cited in note 26, see Irving S. Friedman, Treasury Department, memorandum to A. N. Overby, deputy managing director, IMF, November 2, 1949; Overby to Snyder (with enclosed memoranda by Friedman), December 29, 1949, Snyder Papers, box 20, Alphabetical File, International Monetary Fund folder; Hoffman to Harriman, January 10, 1950, ECA File, box 63; Southard, memorandum to Snyder, January 16, 1950, Snyder Papers, box 11, ECA and International Trade Organization folder, 1950; G. H. Willis, Treasury Department, to Tasca, January 11, 1950, Treasury Records, Acc. 66A1039, box 3, Marshall Plan Correspondence (Official) folder; Acheson to American Embassy, London, January 27, 1950, *FRUS*, 1950, 3: 623-24; "The Outcome of the EPU Negotiations," unsigned memorandum, June 16, 1950, Snyder Papers, box 11, ECA and International Trade Organization folder, 1950; and Hoffman to Katz, Tasca, and Lincoln Gordon, June 17, 1950; Bissell to Harriman, July 8, 1950, ECA File, box 66.

⁶⁹ Marshall to Douglas, February 29, 1948, *FRUS*, 1948, 3 (Washington, D.C., 1974), 384-86; Douglas to Marshall, March 3, 1948, RG 84, box 1035, file 850, Marshall Plan; Marshall to Caffery, March 9, 1948, RG 59, file 840.50 Recovery/3-948; and Marshall, circular telegram, March 10, 1948, *ibid.*, Recovery/3-1048.

⁷⁰ Caffery to Marshall, March 24, 1948, RG 59, file 840.50 Recovery/3-2448; and Caffery to American Embassy, London, March 29, 1948, RG 84, box 1035, file 850, Marshall Plan. Also see Hogan, "Paths to Plenty."

⁷¹ Lovett to Caffery, April 8, 1948, *FRUS*, 1948, 3: 414-17. Also see the sources cited in note 69.

office of director general, appoint a European of political stature to this post, and authorize him to direct the organization's activities. Such reforms, they believed, would help circumvent the "tortuously slow" process of decision making by national delegations and enable the OEEC to develop a *European* point of view. A committee of "country representatives," as Harriman put it, could not provide "effective leadership" because each national delegation would be "inclined to consider first the interests of [its] own country." Real leadership required a director general of "international political position," who could speak for Europe as a whole, "initiate or advocate matters requiring top-level consideration," and deal "on a basis of equality with senior government representatives."⁷²

In October 1948, the OEEC agreed to appoint a Committee of Nine to study organizational reforms and make recommendations early in the next year.⁷³ During ensuing discussions the Belgians and the French generally endorsed Harriman's demand for stronger leadership, and Belgian Prime Minister Paul Henri-Spaak drafted a proposal to enhance the functions of the secretariat and create a high-level ministerial committee with considerable executive authority. But when the Committee of Nine convened in February 1949, Britain submitted an alternative plan that called only for a consultative group of ministers to review the OEEC's work between sessions of the council and the executive committee. The British plan denied the group control over the secretariat and hence any independent status and clear executive responsibilities. It reaffirmed instead the role of the executive committee and the council as the "supreme" bodies of the organization and made clear that all policy decisions must pass through both groups for approval by their national delegations.⁷⁴

The outcome was a compromise approved by the council on February 17. The group of ministers was to be consultative in nature and have no authority to make decisions on its own. But it could meet frequently at the request of its chair, review the work of the organization, discuss "high-level" business, and work closely with the secretariat in the preparation of agenda for sessions of the council and executive committee. The resolution also instructed the council to convene at the ministerial level as often as necessary, but no less than four times a year, and endorsed a recommendation from the secretary general to enlarge the staff and functions of the secretariat.⁷⁵ The Americans were hopeful that additional steps would follow. Richard Bissell, Hoffman's deputy in Washington, thought it especially important that the OEEC act quickly to appoint "highly competent people" to the enlarged staff of the secretariat. The creation of a "competent and disinterested staff of international civil servants," he argued, would help to make the OEEC a "nucleus for greater European unity in the economic field."⁷⁶ In

⁷² Harriman to Marshall and Hoffman, July 31, 1948, ECA File, box 5. Also see Harriman to Marshall, July 17, 1948, RG 59, file 840.50 Recovery/7-1748; Theodore Geiger, memorandum to Bissell, July 17, 1948, RG 286, Acc. 53A405, box 60, AAP Policy Series folder; Lovett to Harriman, July 22, 1948, *FRUS*, 1948, 3: 471-72; and Hoffman to Harriman, September 10, 1948, ECA File, box 45.

⁷³ Harriman to Hoffman, October 16, 1948, ECA File, box 5.

⁷⁴ Ambassador Alan G. Kirk, Belgium, to the secretary of state, January 5, 1949, RG 59, file 840.50 Recovery/1-549; and Harriman to Hoffman, January 10, 1949; Foster to Hoffman, February 10, 1949; Harriman to Hoffman, February 15, 1949, ECA File, box 6.

⁷⁵ Harriman to Hoffman, February 17, 18, 1948, ECA File, box 6.

⁷⁶ Hoffman to Harriman from Bissell, February 18, 1949, *ibid.*, box 46.

addition, Harriman and others continued to press for the creation of a new post of director general and for the appointment to that position "of a leader with sufficient authority, stature and international prestige to influence member governments" toward the sort of economic integration that Congress and the American people expected. The man they had in mind for the job was Henri-Spaak, a leading advocate of European union and of the OEEC as a strong, supranational authority.⁷⁷

But the organizational imperatives of British diplomacy dictated a different course—one hostile to both the new position and Spaak's appointment. As Harriman pointed out, "the reasons why we want Spaak were the very reasons why the British were opposing him, namely, the making of the OEEC [into] an effective organization freed from the present British curb-rein."⁷⁸ Foreign Secretary Ernest Bevin told Ambassador Lewis Douglas in London that his government could not "accept the appointment of Spaak or any other Continental to a position of control in the OEEC," that the OEEC's functions should remain strictly "economic and factual," and that it must not become a European political authority with the power to coordinate national policies across the ERP area. After all, he said, Britain was a world power, "not merely a European power," and "could not accept integration in western Europe on a scale which would impair its other responsibilities."⁷⁹

At meetings in late 1949, the British directed their "best efforts at smothering the [American] proposal." Under their influence, the OEEC's consultative group tentatively agreed to appoint a new official to act as liaison between the OEEC and outside organizations, present its views to world opinion, and harmonize differences between the participating countries. But his specific duties and powers were not clearly defined. He would not have a fully independent status, control over the OEEC's secretariat, or anything but a "consultative" voice in meetings of the council and consultative group.⁸⁰ Nor could he hold another office, either in his own country or in another international organization. Such a stipulation seemed designed to disqualify Spaak, because it would have forced him to resign as a member of the Belgian Parliament and as president of the European Assembly. Secretary of State Dean Acheson pointed out that the compromise did not provide an "acceptable basis for Spaak or any other strong European personality to consider taking [the] position."⁸¹

British opposition hardened in January 1950, after Spaak had publicly criticized British policy toward European unification. Sir Oliver Franks, the British ambassador in Washington, told Acheson that his government could never consent to Spaak's appointment as director general. Acheson would not urge Spaak to

⁷⁷ George Perkins, memorandum to Acheson, September 9, 1949; Acheson to Douglas, October 14, 1949; Douglas to Acheson, October 18, 26, 1949, *FRUS*, 1949, 4: 421–23, 429–30, 430–31, 435–37. Also see Harriman to Hoffman, September 27, 1949, RG 59, file 103.ECA/9–3049.

⁷⁸ Harriman to Acheson, October 12, 1948, Harriman Papers, OEEC folder.

⁷⁹ Douglas to Acheson, October 18, 26, 1949, *FRUS*, 1949, 4: 430–31, 435–37. Also see Ambassador Warren R. Austin to Acheson, from Ambassador Philip Jessup, September 30, 1949, RG 59, file 840.50 Recovery/9–3049.

⁸⁰ Douglas to Acheson, December 1, 1949, *ibid.*, Recovery/12–149. Also see Katz to Hoffman, December 21, 1949, *FRUS*, 1949, 4: 464–67.

⁸¹ Acheson to American Embassy, Paris, December 23, 1949, *FRUS*, 1949, 4: 468. Also see Katz to Hoffman, December 22, 1949, ECA File, box 8.

withdraw his candidacy, as Franks had requested. Nor would he and other American officials abandon their demand for the new position. But given the British opposition, they stopped urging Spaak's appointment and made the Europeans primarily responsible for selecting a candidate and defining his responsibilities.⁸²

This softening of the American position made it possible for European leaders to implement the agreement of late 1949 and to do so as part of a major reorganization of the OEEC in February and April 1950. The consultative group of ministers was abolished, the council and the executive committee were to meet more frequently at the ministerial level, and top officials of the council were to spend more time directing the work of the organization. Most important, the council created the new post of "political conciliator"—as opposed to director general—and elected Dirk Stikker of the Netherlands to this position and to the chairmanship of the council. Stikker was an advocate of European integration, and Harriman considered his appointment a "constructive" step "in strengthening OEEC at the political level." Hoffman heralded it as the "single most hopeful move since the inception of OEEC."⁸³ Despite this hyperbole, Stikker's authority was obviously ill defined, and real power still remained in the hands of individual governments. The reorganization strengthened the OEEC as a vehicle for conventional intergovernmental collaboration. But it stopped short of creating the kind of independent, corporate authority that the ECA considered essential to European integration.

Nor did things change when the Western powers met at the London Foreign Ministers Conference in May 1950. American policy planning for the conference envisioned a more active role for the United States in European economic affairs, to be achieved either by associating the United States with the OEEC or by enlarging the functions of the North Atlantic Treaty Organization (NATO). This was seen as necessary because Britain and France were no longer capable of acting as great powers without American aid and leadership. Only ongoing American involvement, through stronger institutions of coordination and control, would give the Europeans the assurances of support, security, and direction they needed to move forward with plans for harmonizing national policies, unifying economies, and integrating Germany.⁸⁴

The difficulty came in reconciling British and French differences over which organizational framework to use. The French wanted to centralize authority, including authority over national policies, in the hands of a strong collective agency. French Premier Georges Bidault had first expressed this desire in a vague proposal for an "Atlantic High Council."⁸⁵ By the time the foreign ministers met, Bidault's

⁸² Acheson to Harriman, January 24, 1950, *FRUS*, 1950, 3: 616–17; and *New York Times*, January 15, 1950, pp. 1, 19.

⁸³ Harriman to Hoffman, February 2, 1950, ECA File, box 17; Harriman to Hoffman, February 4, 1950, *ibid.*, box 20; Harriman to Hoffman, March 25, 1950, *ibid.*, box 17; Katz to Hoffman, April 4, 1950, *ibid.*, box 18; Harriman to Hoffman, February 4, 1950, *ibid.*, box 20; and Hoffman to Harriman, February 3, 1950, *ibid.*, box 63.

⁸⁴ Memorandum Prepared in the Bureau of German Affairs, [February 11, 1950]; Memorandum of Conversation by the Officer in Charge of United Kingdom and Ireland Affairs (Wayne G. Jackson), March 7, 1950, *FRUS*, 1950, 3: 638–42; and Joseph Jones to George Elsey (and enclosed memorandum by Jones, dated March 2, 1950), March 3, 1950, HSTL, George M. Elsey Papers, box 62, Foreign Policy Planning folder.

⁸⁵ Ambassador David K. E. Bruce, Paris, to Acheson, April 20, 15, 1950, *FRUS*, 1950, 3: 57–58, 54–55.

proposal had given way to more concrete French initiatives. One of these was the Schuman Plan for a European coal and steel community that would blend German and French interests under the guiding hand of a common authority. Acheson later recalled how the prospect of such a community without Britain hung like a pall over the deliberations. According to the records, however, the foreign ministers barely touched on the plan during their meetings.⁸⁶ Controversy turned instead on the French search for an organizational framework that would bring the British into a Continental group and facilitate cooperation between this group and the United States in economic and political matters of common concern. This could be achieved, the French suggested, by making the United States an associate member of the OEEC. The latter would remain a "European organization for purely European affairs," but its transatlantic link would deepen the American commitment in Europe, and this, together with Britain's active involvement there, would pave the road for Germany's further revitalization and reintegration.⁸⁷

Yet the British steadfastly refused to be incorporated into a purely European "zone" that made no allowances for their longstanding commitments around the world and ignored their claim to a special relationship with the United States. They preferred a North Atlantic rather than a European framework—one that would embrace the United States, the United Kingdom, and "some European entity" and that could be brought about by developing NATO as an "umbrella" organization with separate arms for military and nonmilitary affairs. Such an arrangement, they insisted, would recognize Britain's three-cornered commitment to Europe, Commonwealth, and United States. It would also permit Anglo-American cooperation in areas outside of Europe and yet allow the Germans to collaborate in nonmilitary affairs of the North Atlantic community. Together with Anglo-American military guarantees under the North Atlantic Pact, it would actually absorb and contain German power, insure French security, and thus open the door for European union and German reintegration.⁸⁸

The French, however, viewed things in a different light. They saw in such a proposal an alarming degree of British aloofness that would virtually guarantee German predominance in an integrated European economy. Nor were they inclined to use NATO as an organizing framework, a course, they insisted, that would lead to Germany's rearmament and preclude participation by European neutrals. The result, according to French Foreign Minister Robert Schuman, would be "new iron curtains on our side of [the] present Iron Curtain." With this Acheson seemed to agree.⁸⁹ And the outcome was a compromise that pointed in all

⁸⁶ U.S. Delegation at the Tripartite Foreign Ministers Meeting to Webb, May 12, 1950, *ibid.*, 1044–51; and Acheson, *Present at the Creation*, 393.

⁸⁷ U.S. Delegation at the Tripartite Foreign Ministers Meeting to Webb, May 16, 1950, *FRUS*, 1950, 3: 1069–71. Also see U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, May 6, 1950; Acheson to Webb, May 9, 1950; U.S. Delegation at the Tripartite Foreign Ministers Meeting to Webb, May 11, 1950; Acheson to Webb, May 14, 1950, *ibid.*, 911–13, 1013–18, 1040–43, 1061–67.

⁸⁸ U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, April 25, 1950, *ibid.*, 860–63. Also see U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, April 26, May 6, 1950; Acheson to certain diplomatic offices, April 27, 1950; U.S. Delegation at the Tripartite Foreign Ministers Meeting to Webb, May 9, 1950; Acheson to Webb, May 14, 1950, *ibid.*, 881–83, 911–13, 71–72, 1018–22, 1061–67.

⁸⁹ Acheson to Webb, May 9, 1950, *ibid.*, 1013–18; and U.S. Delegation at the Tripartite Foreign Ministers

directions. Acheson pledged continuing interest in European economic problems; Bevin and Schuman called for a "working relationship" between the United States and the OEEC. This relationship would be "informal" and would detract neither from the OEEC's role as an agency "devoted primarily to European economic problems" nor from NATO's general responsibility for economic and political issues affecting the North Atlantic area. American leaders tried to put a good face on the compromise. The American delegation in London considered it the best possible, and Harriman thought it another step forward. The Europeans had at least agreed on the importance of continued economic collaboration and on the need to give this collaboration "organizational expression." In June, moreover, they brought the United States into the OEEC as an associate member.⁹⁰

Although this development further strengthened the OEEC as an agency of intergovernmental cooperation, it still fell short of what American leaders wanted—central institutions that could transcend European sovereignties and coordinate national policies. This policy did not belie their faith in the integrating powers of the market. Rather, it reflected their conviction that economic integration was only possible between countries with roughly congruent monetary and fiscal policies and that achieving this congruence required supranational organizations of regulation and control. Through such organizations they had sought to build a unified system poised between British and German power and capable of producing abundance. Their efforts, however, kept foundering on the imperatives of British diplomacy, and the kind of Anglo-American compromise that had led to British participation in the European Payments Union could not be used to resolve the disputes over the OEEC and the Managing Board in a way that reconciled British commitments with American hopes for an integrated Europe.

NONETHELESS, AMERICAN MARSHALL PLANNERS had scored substantial gains since the inception of the recovery program nearly four years earlier. They had helped revive European production, stabilize currencies, liberalize trade and payments, and restore a measure of social peace and prosperity to Europe. They had done so by urging European leaders to cast off old habits of bilateralism and restrictionism, old concerns with self-sufficiency and autonomy, old traditions of class conflict and national rivalry, and old ways of doing business. All of these were seen as barriers to maximum productivity, higher living standards, lower prices, and equilibrium between the dollar and nondollar worlds. And they were to give way now to a new European economic community founded on the principles of American neo-capitalism.

These principles were not a prescription for laissez-faire capitalism. While there was room in the American plan for such free-market strategies as full convertibility

Meeting to Webb, May 11, 1950, *ibid.*, 1040–43. Also see U.S. Delegation at the Tripartite Preparatory Meetings to Acheson, April 29, 1950, *ibid.*, 896–98; and Acheson to Webb, May 9, 1950, *ibid.*, 1013–18.

⁹⁰ For the final communique issued by the ministers, see Department of State *Bulletin*, May 29, 1950, p. 827. Also see U.S. Delegation at the Tripartite Foreign Ministers Meeting to Webb, May 16, 1950; Harriman to Acheson, June 15, 1950, *FRUS*, 1950, 3: 1069–71, 662–63; Harriman to Acheson, June 2, 1950, Harriman Papers, OEEC folder; and Katz to Hoffman, June 30, 1950, ECA File, box 18.

and multilateralism and for such free-market principles as competition and comparative advantage, there was also room for government aid and action, public-private cooperation, and central institutions of coordination and control. Through their trade and payments initiatives, their technical assistance programs and productivity teams, and their support for a stronger OEEC, American leaders tried to forge an organic economic order in Europe. The new order would be balanced between a paternalistic statism and the old capitalism, would be tied together by market forces and institutional coordinators, and would be founded on economic interdependence and supranationalism, American mass-production techniques, and American labor-management programs for a productive abundance in which all could share. A similar order had supposedly cleared a path to social harmony and affluence in the United States. And once in operation, or so the Americans believed, it would also lead to a new era of peace and plenty on the Continent.

The new era, to be sure, had not come by the end of 1950. The British had refused to join the movement for economic integration. In addition, the declining volume of American aid, the shift from economic to military priorities after the outbreak of the Korean War, and the shortages and inflationary pressures that accompanied the rearmament effort, all combined to reduce the leverage that American officials could use to integrate Europe. They also discouraged further steps to liberalize trade and payments on the Continent and revived old disputes over how best to harmonize national policies and divide resources. By that time, however, American Marshall planners had helped generate a vision of redemption on the Continent, one that saw a unified Western Europe arising from the rubble and the ruin of war, like Lazarus from the grave, with new life and productive vitality, and that led to the Schuman Plan of 1950, the European Coal and Steel Community of 1951, and ultimately the Common Market.

Prophecy and Discovery: On the Spiritual Origins of Christopher Columbus's "Enterprise of the Indies"

PAULINE MOFFITT WATTS

And I saw a new heaven and a new earth: for the first heaven and the first earth
were passed away; and there was no more sea.

—Revelations 21:1

God made me the messenger of the new heaven and the new earth of which he
spoke in the Apocalypse of St. John after having spoken of it through the mouth
of Isaiah; and he showed me the spot where to find it.

—Christopher Columbus, 1500¹

CHRISTOPHER COLUMBUS WANTED TO BE REMEMBERED as the Admiral of the Ocean Sea. It was a formal title that he had struggled to win from his king and queen, Ferdinand and Isabella of Spain, in recognition of his discovery and exploration of the Indies. And for the last one hundred fifty years scholars have almost without exception elaborated the image of Columbus as the bold and innovative explorer who, armed with a "rational" or "scientific" geography, battled the ignorance and superstition of influential ecclesiastics at the Aragonese court until he finally won royal support for his "Enterprise of the Indies." Samuel Eliot Morison's well-known biography, *The Admiral of the Ocean Sea* (1942), is a prime example of this traditional line of scholarly interpretation.²

Morison's image of Columbus coincides with but one of two images that

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¹ This passage occurs in a letter written by Columbus to a member of the royal court in 1500, after his third voyage when he was returned from the New World Indies to Spain in chains; G. B. Spotorno, *Memorials of Columbus* (London, 1823), 224.

² After almost seven years of petitioning at the Spanish royal court, Columbus won acceptance for his Enterprise of the Indies—his plan to reach the oriental archipelago by sailing west—from Queen Isabella in

Columbus had of himself. The second image tended, particularly in his later years, to take precedence over the first and seemed to consume him. He came to believe that he was predestined to fulfill a number of prophecies in preparation for the coming of the Antichrist and the end of the world. According to his calculations, these events were not far off. This second self-image is epitomized in the signature that Columbus adopted: Christoferens. It is an awkward latinization of his given name and means "Christ-bearer." Until recently, little attention has been paid to Christoferens—that is, to the spiritual dimension of Columbus's personality, to the religious and cultural environment out of which it developed, and to its possible influence on the genesis of his voyages of discovery.³ Yet Columbus's apocalyptic vision of the world and of the special role that he was destined to play in the unfolding of events that would presage the end of time was a major stimulus for his voyages. Moreover, his apocalypticism must be recognized as inseparable from his geography and cosmology if a balanced picture of the historical significance of his Enterprise of the Indies is to be achieved.

The origins and development of the two-fold conception that Columbus had of himself, and of the sense of mission that grew out of it, are not easy to trace. Details regarding his background and education are sparse. According to his son Ferdinand's biography and to Bartolomé de Las Casas's *Historia de las Indias*, the two principal early sources for the mariner's life, Columbus studied for a while at the University of Pavia. There, Ferdinand wrote, he "studied enough . . . to understand the geographers, of whose teaching he was very fond; for this reason he also gave himself to the study of astronomy and geometry, since these sciences are so closely related that one depends upon the other. And because Ptolemy, in the beginning of his *Geography*, says that one cannot be a good geographer unless one knows how to draw too, he learned drawing, in order to be able to show the position of countries and form geographic bodies, plane and round."⁴ Scholars have long doubted that Columbus studied at the University of Pavia. Many follow instead the argument advanced by Cornelio Desimoni in 1894 that Pavia refers not to the famous Italian

1492. For at least a year prior to that acceptance he insisted that he be granted the title Admiral of the Ocean Sea and a generous portion of all future profits from any lands that he might discover as conditions for undertaking his voyage. These demands almost cost him the royal support he so persistently sought. See Ferdinand Columbus, *The Life of the Admiral Christopher Columbus by His Son Ferdinand*, trans. Benjamin Keen (New Brunswick, 1959), chap. 14. It should be noted that this translation was made from the first Italian edition of Ferdinand's biography, published in Venice in 1571; the Spanish original is lost. See Samuel Eliot Morison, *Admiral of The Ocean Sea*, 1 (Boston, 1942), chap. 8. For the texts of the seven documents signed by Columbus and the Spanish monarchs regarding the Enterprise of the Indies, see Samuel Eliot Morison, *Journals and Other Documents on the Life and Voyages of Christopher Columbus* (New York, 1963).

³ Columbus first signed himself Christoferens in a 1493 memorandum addressed to Ferdinand and Isabella. On virtually everything he signed from 1493 until his death in 1506 he used the following sigil, which has never been definitively deciphered:

.S.
.S. A .S.
X M Y
Xpō FERENS.

On this curious sigil and the various attempts to decode it, see Paolo Emilio Taviani, *Christophe Colomb: Genèse de la grande découverte*, 2 (Paris, 1980), 38–40. Taviani provided an excellent bibliography of the relevant scholarship; *ibid.*, 45. Also see Alain Milhou, *Colón y su mentalidad mesiánica en el ambiente franciscanista español*, Cuadernos Colombinos, vol. 11 (Valladolid, 1983), 59–90.

⁴ Ferdinand Columbus, *Life of the Admiral Christopher Columbus*, chap. 3, p. 9. On Columbus's education, compare Bartolomé de Las Casas, *Historia de las Indias*, ed. José Sancho Rayón, 1 (Madrid, 1875), 46.

university but rather to the Vicolo Pavia—an alleyway in Genoa where, in the mid-fifteenth century, the guild of the wool workers ran a well-known school for the children of their members. Columbus's father was a wool maker in Genoa.⁵ While the question of where and when Columbus received his education remains unresolved, evidence regarding some of the more important sources of his thought survives.

Columbus apparently acquired much of his knowledge of geography, cosmology, history, astronomy, and other related subjects from a number of popular and quite widely diffused compilations that he read and annotated. Prominent among them were Pliny the Elder's *Naturalis historia*, Plutarch's *Lives*, Marco Polo's *Il milione*, the *Historia rerum ubique gestarum* of Pius II, and Pierre d'Ailly's *Imago mundi*. In these works Columbus would have encountered the following names: Aristotle, Averroes, Avicenna, Eratosthenes, Marinus of Tyre, Strabo, Ptolemy, Solinus, Seneca, Julius Capitolinus, Flavius Josephus, Augustine, Ambrose, Isidore of Seville, the Venerable Bede, Alfraganus, Roger Bacon, John Mandeville, Joachim of Fiore, Thomas Aquinas, Peter Comestor, Nicholas of Lyra, Francis Mayronnes, and Paolo Toscanelli. The depth of Columbus's knowledge of these figures would have varied greatly had it been limited to these compilations; some are barely mentioned, while the views of others are described in considerable detail. Finally, it is important to bear in mind that Columbus, like most Christians of his age, was deeply and thoroughly acquainted with the Old and New Testaments and with the Apocrypha (which would have been included in the Vulgate version of the Old Testament that he used).⁶

Without doubt, Columbus did not have the advanced, specialized education of a professional academic. But he did read and annotate works composed in Latin (for example, d'Ailly's *Imago mundi* and Pius II's *Historia*), Castilian (Alfonso de Palencia's translation of Plutarch's *Lives*), and Italian (Cristoforo Landino's translation of Pliny's *Naturalis historia*). His script was clear and not unsophisticated. As Ferdinand noted and Las Casas repeated, "So fine was his hand that he might have earned his living by that skill alone." His interest in history, geography, astronomy, and cosmology and his readings in these subjects were likely shared by many of the better-educated merchants, navigators, bankers, and business entrepreneurs of his day. And this is precisely what Columbus was—an experienced sailor and sometime merchant who married into a family belonging to the minor nobility of Portugal, a man whose fortunes had prospered on a modest scale.⁷

⁵ Desimoni, *Questioni colombiane*, in Cesare de Lollis, ed., *Raccolta di documenti e studi pubblicati della R. Commissione Colombiana*, pt. 2, vol. 3 (Rome, 1894), 29. For a summary of the debate over whether Columbus studied at the University of Pavia, see Taviani, *Christophe Colomb*, 60–62. Las Casas's history seems to support Desimoni's theory, for Las Casas wrote that Columbus had an elementary rather than an advanced level of instruction. Columbus "estudió en Pavia los primeros rudimentos de letras"; *Historia de las Indias*, 46.

⁶ Columbus's copies of the following works survive: d'Ailly's *Imago mundi* (1480 or 1483), Pius II's *Historia rerum ubique gestarum* (1477), Marco Polo's *De consuetudinibus et conditionibus orientalium regionum* and a resume of it in Italian (*Il milione*) by Pipino of Bologna (1485), Pliny's *Naturalis historia* (the Italian translation by Cristoforo Landino, Venice, 1489), Plutarch's *Lives* (in a 1491 Castilian translation by Alfonso de Palencia), and a fifteenth-century palimpsest of Seneca's *Tragedies*. On the sources to which Columbus had access, see Ferdinand Columbus, *Life of the Admiral Christopher Columbus*, chaps. 6–9; Las Casas, *Historia de las Indias*, chaps. 6–11; Taviani, *Christophe Colomb*, 231–38; and S. de La Rosa, *Libros y autógrafos de Cristóbal Colón* (Seville, 1891).

⁷ Ferdinand Columbus, *Life of the Admiral Christopher Columbus*, chap. 3, p. 9; and Las Casas, *Historia de las*

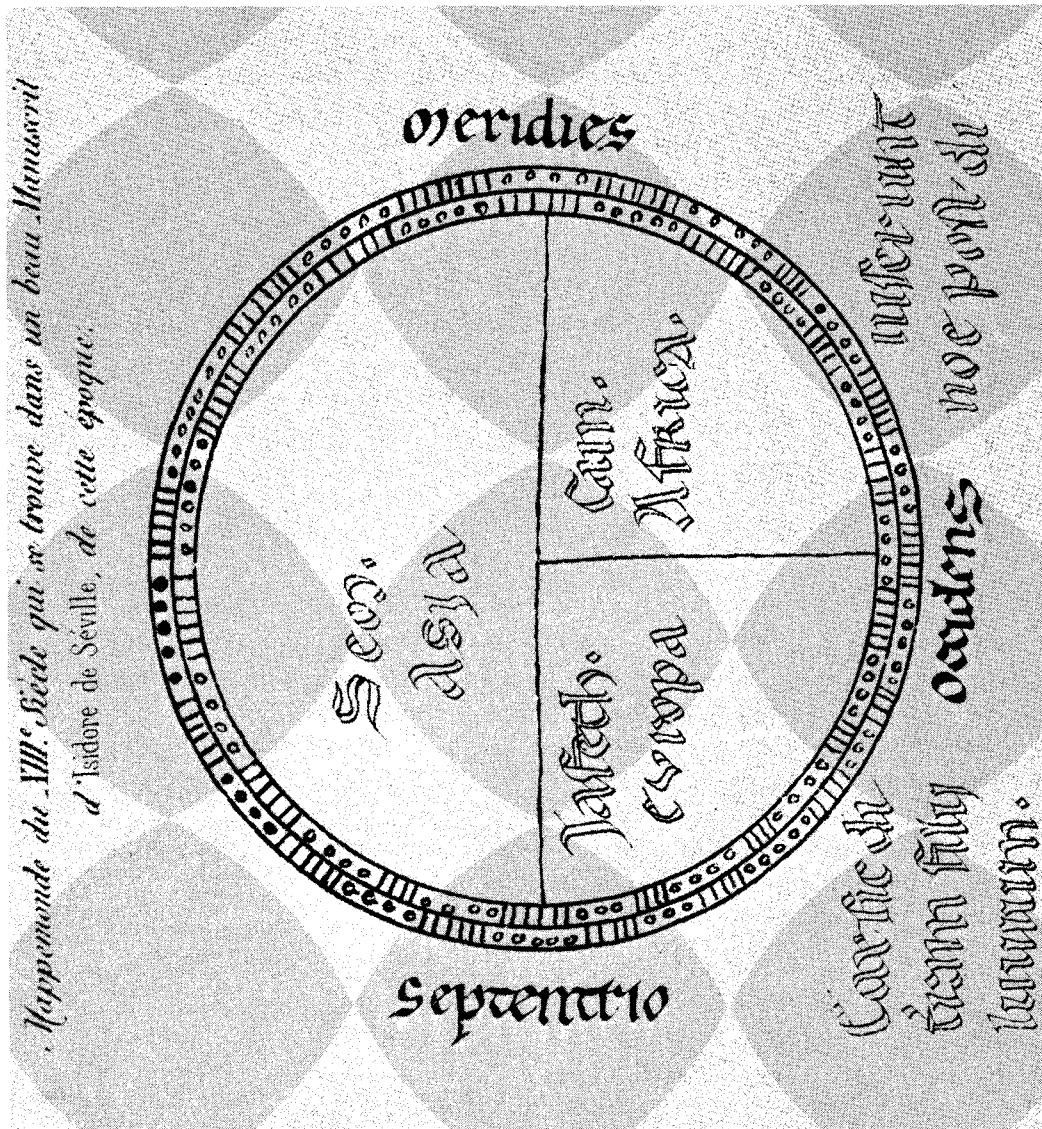


Figure 1: T-O Map of the Thirteenth Century. Diagram reproduced from Vicomte de Santarem, *Atlas composé de mappemondes, de Portulans, et de cartes hydrographiques et historiques depuis le VI^e jusqu'au XVI^e siècle* (Paris, 1849), plate 5, no. 11.

The vision of the cosmos that Columbus shared with many of his contemporaries was derived from a variety of well-known ancient and medieval sources, including many by the authors mentioned above. It seems likely that Columbus's vision came principally from d'Ailly's *Imago mundi* and opuscula and from Pius II's *Historia*. From these works he arrived at a composite picture of a cosmos that was finite and geocentric. The world was located at the center of the seven concentric planetary spheres, themselves enclosed by the outermost shell of the fixed stars. The composition of the heavens and the movements of the planets and stars were permanent and unchanging and could and did influence actions and events on the earth, which was always subject to change—to generation and corruption.

The world itself was commonly depicted as a disk divided into three parts: Europe, Asia, and Africa. This disk was circumscribed by a band of ocean, the impassable sea whose unknown expanses had been feared since antiquity. The tripartite division of the world corresponded to the known land masses and was corroborated by a passage from the Old Testament. "The sons of Noah who went forth from the ark were Shem, Ham, and Japheth. . . . These three were the sons of Noah; and from these the whole earth was peopled" (Gen. 9:19). Accordingly, the most common form of the medieval *mappaemundi*, the so-called T-O map, was a circle, with the tripartite division resulting from the placing of the "T" within the circle "O." Since Shem was considered to be the eldest of Noah's sons, his domain was the largest of the three land masses, Asia. Ham's portion was Africa, and Japheth's was Europe (see Figure 1).

East was at the top of most medieval *mappaemundi*. There the terrestrial paradise was located, again in accordance with Scripture. "And the Lord God planted a garden eastward in Eden and there he set the man he had formed" (Gen. 2:8). Frequently the terrestrial paradise was depicted as an island, separated from the world by the waters of the flood. On the northern shores of Africa were the "monstrous races," first catalogued by Pliny and subsequently incorporated into various popular medieval Christian legends. Somewhere in the northern stretches of Asia, according to these legends, were the tribes of Gog and Magog, imprisoned within walls constructed by Alexander the Great. These tribes would be loosed upon mankind when the end of the world was imminent, as prophesied in the *Revelation* of John of Patmos and other influential apocalyptic visions, such as that of the pseudo-Methodius. Until these events occurred, the cosmos was the place where men and women were fated to live after Adam and Eve had been cast out of the Garden of Eden. This was the setting of fallen humanity's struggle to redeem itself in the eyes of its Maker and to recover the lost paradise so prominently and so nostalgically depicted on so many *mappaemundi* (see Figure 2).⁸

Indias, 46. On Columbus's knowledge of Spanish, Italian, and Latin, see Taviani, *Christophe Colomb*, 42–43, 45; and V. I. Milani, "The Written Language of Christopher Columbus," in *Forum Italicum* (Buffalo, 1973). On the connections among literary, scientific, and mercantile cultures during the age of discoveries, see J. H. Parry, *The Age of Reconnaissance: Discovery, Exploration, Settlement, 1450–1650* (Berkeley and Los Angeles, 1981), pt. 1; Boies Penrose, *Travel and Discovery in the Renaissance, 1420–1620* (New York, 1962), chaps. 16–17; Leonardo Olschki, *Storia letteraria delle scoperte geografiche* (Florence, 1937); and Irving Leonard, *Books of the Brave* (New York, 1964).

⁸ On medieval conceptions of geography—"spiritual" and otherwise—and their representations in *mappaemundi*, see Milhou, *Colón y su mentalidad mesiánica*, 403–11; W. G. L. Randles, *De la terre plate au globe*



Figure 2: Ebstorf World Map of the Thirteenth Century.
Facsimile reproduced from Konrad Miller, *Die Ältesten Weltkarten* (Stuttgart, 1895–98), vol. 5

Men saw themselves essentially as *viators*, “strangers and pilgrims” St. Francis called them, journeying through the time and space of a tainted cosmos in search of a redemption that would occur only with the termination of that cosmos. These journeys were individual and collective. Sometimes they were voluntary, sometimes not. The peregrinations of the body were often inextricable from those of the soul. The *ecclesia peregrina* of Augustine’s *The City of God*, the *imrams* of the Irish saints who set themselves adrift at sea, the Crusades, the lengthy pilgrimages undertaken by many thousands of nameless men and women to the grid of sacred sites that

terrestre: Une mutation épistémologique rapide, 1480–1520 (Paris, 1980); George H. T. Kimble, *Geography in the Middle Ages* (New York, 1938), chap. 8; John K. Wright, *Geographical Lore of the Time of the Crusades* (New York, 1925), pt. 1, chap. 4; and John Block Friedman, *The Monstrous Races in Medieval Art and Thought* (Cambridge, Mass., 1981), chap. 3. For two fundamental catalogues of medieval *mappaemundi*, see Konrad Miller, *Mappaemundi: Die Ältesten Weltkarten*, 6 vols. (Stuttgart, 1895–98); and Marcel Destombes, *Mappemondes, A.D. 1200–1500: Monuments cartographiques anciens* (Amsterdam, 1964).

stretched across Europe to the Holy Land, the deliberate dependence of mendicant orders on the randomly encountered charity of others—all are manifestations of the symbiotic relationship between the internal and external journeys necessarily undertaken by post-lapsarian man.

Guided more often than not by prophecies regarding the appearance of an emperor-messiah, the conversion of all the peoples of the world to Christianity, the final recovery of the Holy Land from the infidel, and the advent of the Antichrist, Columbus and his contemporaries sought to discover and play out their historical roles in a cosmic drama they perceived as inexorably unfolding from the moment that Adam and Eve had been expelled from the Garden of Eden. Their searches and their enactments produced proto-nationalistic “duels of prophecies” and could also result in a civic apocalypticism, such as that of Savonarola of Florence, the quintessential urban prophet. But the drama took place on a larger stage as well, encompassing less particularized experiences of time, space, and place.⁹ The striving to fulfill prophecy on a cosmic or global scale was a major stimulus to travel and discovery, from the early Franciscan missions into Asia to Columbus’s Enterprise of the Indies, which led to his discovery of “a new heaven and a new earth” in the Americas.

MODERN STUDY OF THE ORIGINS of Columbus’s Enterprise of the Indies can be traced back to the first half of the nineteenth century. In the years 1836–39, Alexander von Humboldt published his *Examen critique de l’histoire de la géographie du Nouveau Continent*, an important five-volume study of European discovery and exploration of the Americas during the fifteenth and sixteenth centuries. In the first volume of that work Humboldt wrote what remains the classic analysis of how and why Columbus made his famous voyages. Humboldt recognized the “persuasive force” of a “mystical theology,” which increasingly gripped the “great soul” of Columbus, but he was basically puzzled by this “strange combination of ideas and sentiments in a superior man, gifted with a high intelligence and with an invincible courage in adversity, nourished on scholastic theology, very apt, however, in the management of business, of an ardent and sometimes disorderly imagination, unexpectedly ascending from the simple and naive language of a sailor to felicitous poetic inspirations, reflecting in himself at the same time, so to speak, everything sublime and bizarre that the middle ages produced.”¹⁰ In spite of his uncertainties regarding the religious dimension of Columbus’s personality, Humboldt took greater pains to accommodate it than did any of his successors. He argued that the

⁹ On the medieval image of the *viator*, see Gerhart Ladner, “*Homo viator*: Medieval Ideas on Alienation and Order,” *Speculum*, 42 (1967): 233–59; and Jonathan Sumption, *Pilgrimage: An Image of Medieval Religions* (Totowa, N.J., 1975). On medieval apocalypticism, see Norman Cohn, *The Pursuit of the Millennium* (New York, 1961); Marjorie Reeves, *The Influence of Prophecy in the Later Middle Ages: A Study in Joachimism* (Oxford, 1969); and Bernard McGinn, *Visions of the End: Apocalyptic Traditions in the Middle Ages* (New York, 1979). On “civic” apocalypticism, see Donald Weinstein, *Savonarola and Florence: Prophecy and Patriotism in the Renaissance* (Princeton, 1970).

¹⁰ Humboldt, *Examen critique de l’histoire de la géographie du Nouveau Continent et du progrès de l’astronomie nautique aux quinzième et seizième siècles*, I (Paris, 1836), 110.

mariner was heir to a long medieval tradition of a "géographie mystique," which he traced in considerable detail.¹¹ In Humboldt's judgment, Christofereans represented a discomfiting but probably inevitable medieval residue in the great explorer's mind that rose to the surface as his rational powers declined in his unhappy waning years. Humboldt clearly felt most comfortable with the conclusion that Columbus's voyages were actually based on a "scientific" geography gleaned from a variety of ancient and medieval sources.

In writing his account from these sources, Humboldt closely followed chapters 6–9 of Ferdinand's biography in which Ferdinand discussed his father's reasons for believing that he could reach the Indies by sailing west. According to Ferdinand, Columbus believed that the world was a sphere, that it could be circumnavigated, and that the only unexplored region was the area extending from the eastern shores of Asia to the Azores. Following ancient authorities, such as Aristotle, Pliny, Seneca, Solinus, and Julius Capitolinus, Columbus argued that the extent of this unknown region was not as great as commonly supposed. But Ferdinand suggested and Humboldt strove to demonstrate that the most important sources of Columbus's cosmographical knowledge and the principal stimuli for his voyages were *Imago mundi* by d'Ailly, the French cardinal and conciliarist, and two letters purportedly written to him by Paolo Toscanelli, the famous Florentine physician and astronomer.¹²

Two serious problems impede any assessment of the historical significance of the Toscanelli-Columbus correspondence. The first problem concerns the authenticity of the two letters received by Columbus and the dating of the correspondence. The earlier, and by far the more significant, of the two letters is in large part a copy of a letter, dated June 25, 1474, that Toscanelli sent to Alfonso V of Portugal via his friend Ferdinand Martins. It apparently included a map, which remains lost in spite of numerous attempts to locate it. The second letter seems to have been written soon after the first, in response to one that Toscanelli received from Columbus. Considerable controversy surrounds the dating of the letters. Some scholars (for example, Humboldt) have placed them within two years of the Toscanelli letter to Alfonso V. Others, such as Henri Harsse and Henry Vignaud, have placed them later, in 1480 or 1481.¹³

¹¹ *Ibid.*, 110–206.

¹² For Humboldt's discussion of Toscanelli and Columbus, see *ibid.*, 210–56, esp. 255. Humboldt concluded that Toscanelli "was, as Ferdinand Columbus said, the most powerful cause of the spirit [*anima*] with which the admiral launched himself upon the immensity of an unknown sea; strangely, posterity has almost forgotten the influence of this Florentine mathematician." On Columbus and d'Ailly, see *ibid.*, 60–70. Humboldt began his discussion by stating, "Among the authors that Columbus consulted . . . no one is cited with greater predilection than the Cardinal, Pierre d'Ailly." The statistics bear out Humboldt's assertion; there are 898 annotations on d'Ailly's *Imago mundi* in Columbus's hand, more than he devoted to any other single work; Taviani, *Christophe Colomb*, 233. Humboldt did not know Las Casas's biography, which was not edited until 1875. On Columbus's sources, see Ferdinand Columbus, *Life of the Admiral Christopher Columbus*, chaps. 6, 7; and Edmond Buron, *Imago mundi de Pierre d'Ailly*, 3 vols. (Paris, 1930), 1: 206–15. Ferdinand Columbus cited "the conclusion of the second book" of Aristotle's *De caelo et mundo*, the second book of Pliny's *Naturalis historia*, the first book of Seneca's *Quaestiones naturalis*, chapter 68 of Solinus's *Collectanea rerum memorabilium*, and chapter 19 of Julius Capitolinus's *Geographia* as sources for Columbus's belief that the extent of the western ocean was not so great as commonly supposed; *Life of the Admiral Christopher Columbus*, 17–18.

¹³ For Vignaud's description of the various versions of the two letters and the different dates assigned to them, see his *Toscanelli and Columbus: The Letter and Chart of Toscanelli* (New York, 1902), 9–20. He gave the texts

No originals of the Toscanelli-Columbus correspondence survive. There is a Spanish translation of the letters (which were written in Latin) in Las Casas's *Historia* and a sixteenth-century Italian translation of them based on Ferdinand's lost Spanish text. In 1871 Harrisse discovered a Latin version of the first letter written in Columbus's own hand on a blank page of his copy of a 1477 edition of Pius II's *Historia*. That Columbus copied out Toscanelli's letter, however, is not proof that Toscanelli actually sent it to him. Because the originals do not survive and there is no other evidence of an exchange of letters between Toscanelli and Columbus, the authenticity of the correspondence has on occasion been hotly contested. The most famous challenge is that of Vignaud, put forth in a torrent of articles and several books published around the turn of this century. On the basis of what is currently known, it seems extremely unlikely that Toscanelli and Columbus corresponded directly, although Toscanelli wrote most of the first letter and Columbus no doubt read it.¹⁴

The second problem that arises out of the Toscanelli-Columbus correspondence concerns the content. The letters are general in nature: they evoke the marvels of the East, argue that the Orient can be reached more quickly by sailing westward rather than by traveling overland eastward, and provide various calculations in support of the opinion that the stretches of unknown sea to the west are relatively short. Humboldt's analysis of the correspondence has withstood the test of time. Although he stressed the importance of the Toscanelli letters, Humboldt was careful to point out that Columbus could have gotten virtually all of the information contained in them from other sources available to him, particularly the works of Marco Polo, Nicolò de Conti, and d'Ailly.¹⁵ It is difficult to escape the conclusion that the drama inherent in a direct exchange between two such famous figures on the eve of the discovery of the Americas has hitherto diverted scholars from a sober evaluation of the letters themselves in relation to other sources at Columbus's disposal. Toscanelli's influence on Columbus was probably inspirational or corroborative, perhaps both.

D'Ailly's *Imago mundi*, the other traditional source for Columbus's Enterprise of the Indies, is far from being original or forward-looking. Written in 1410, this work is a compendium of ancient and medieval cosmology and geography, intended for the instruction of the ordinary lay reader, which apparently circulated quite extensively in Western Europe during the fifteenth century. Borrowings from

of each version along with an English translation; *ibid.*, appendixes A–E, pp. 275–327. Taviani provided a clear, balanced presentation of the complicated controversy regarding the Toscanelli-Columbus correspondence and included an extensive bibliography; *Christophe Colomb*, 190–211.

¹⁴ For the Spanish version, see Las Casas, *Historia de las Indias*, 92 *passim*. For the Italian version, see *Historie del S. D. Fernando Colombo* (Venice, 1571), f. 16r. For a facsimile of the Latin version in Columbus's hand, see Harrisse, *Biblioteca Vetusissima, Additions* (Paris, 1872), 16–18; and Cesare de Lollis, *Raccolta di documenti e studi pubblicati della R. Commissione Colombiana*, part 1, volume 3: *Autografi di Cristoforo Colombo* [hereafter, *Autografi*] (Rome, 1892), plate 63. Vignaud's attempt to disprove the authenticity of the Toscanelli-Columbus correspondence is a major theme of his *Toscanelli and Columbus* and his *Histoire critique de la grande entreprise de Christophe Colomb* (Paris, 1911).

¹⁵ Humboldt, *Examen critique*, 213–16. Humboldt thought that Nicolò de Conti might well have been a major source for Toscanelli's account of the "Marvels of the East," either through conversation or through book 4 of Poggio Bracciolini's *Historiae de varietate fortunae*, which contained Nicolò's account of his travels. According to Humboldt, Nicolò is one of the anonymous travelers to the East mentioned in Toscanelli's second letter.

other sources, particularly Roger Bacon's *Opus maius*, are evident throughout *Imago mundi*, and *Opus maius* was itself a compendium of thirteenth-century knowledge. What we have here is not esoterica but a lineage of texts containing well-known and widespread sorts of information.¹⁶

Columbus carefully read and annotated d'Ailly's *Imago mundi*. The incunabulum that he used survives in the Biblioteca Colombina in Seville, Spain. Examination of Columbus's copy of *Imago mundi* reveals that chapter 8, "De quantitate terrae habitabilis," is heavily annotated. For this reason, Columbus scholars have consistently pointed to it as central to an understanding of the intellectual origins of Columbus's project. Humboldt demonstrated that chapter 8 is cribbed from Bacon's *Opus maius*; whether Columbus knew that is unclear. Scholars have long assumed that Columbus did not know of Bacon's existence since he nowhere mentioned him by name. But this assumption is incorrect, for Columbus did mention Bacon in his marginalia to another work by d'Ailly contained in the same incunabulum as *Imago mundi*. Whether he knew that in reading d'Ailly he was frequently reading Bacon is another matter that is difficult, if not impossible, to resolve.¹⁷

The Bacon-d'Ailly argument of chapter 8 that so interested Columbus again concerned the relatively short width of the western ocean. Authorities such as Aristotle, Seneca, and Pliny are cited in support of the conclusion that the ocean does not cover three-fourths of the earth's surface, as some had supposed. Conclusive proof is supplied by a passage from the Apocrypha, where it is written that six-sevenths of the world is dry (therefore inhabitable) and only one-seventh is covered by water (2 Esd. 6:42).¹⁸

Columbus's theoretical basis for the assumption that the western ocean was not vast was borne out by his practical experience as a sailor. According to a note in his own hand in his copy of *Imago mundi*, Columbus navigated by the erroneous calculations of the tenth-century Arabian astronomer Alfraganus. Alfraganus, whose figures were recorded by Bacon in *Opus maius* and incorporated into *Imago mundi* by d'Ailly, underestimated the length of a degree at the equator. Using Alfraganus's figure of 56 ²/₃ land miles per equatorial degree (the correct figure is 60 land miles per equatorial degree), Columbus estimated that he had only to sail approximately twenty-five hundred miles westward from the Canary Islands in order to reach the Orient. In two letters written to Ferdinand and Isabella, one

¹⁶ The text of d'Ailly's *Imago mundi* and Columbus's annotations on it have been edited and translated into French by Buron; *Imago mundi de Pierre d'Ailly*. A facsimile of the incunabulum of d'Ailly's works that Columbus used has been published. See Massachusetts Historical Society, *Imago mundi by Petrus de Aiaco (Pierre d'Ailly) with annotations by Christopher Columbus* [hereafter, d'Ailly, Incunabulum] (Boston, 1927). For a listing of the manuscripts and the incunabulum of the *Imago mundi*, see Ludovico Salembier, *Petrus de Alliaco* (Lille, 1886), xxiii.

¹⁷ Humboldt, *Examen critique*, 60–70; Buron, *Imago mundi de Pierre d'Ailly*, 1: chaps. 2, 3. Columbus mentioned Bacon in an annotation on d'Ailly's *Elucidarium astronomice concordie cum theologia et hystorica veritate*; f. 129v; d'Ailly, Incunabulum. In the course of a discussion of the month in which the world was created, d'Ailly remarked that Bacon stated in a letter to Pope Clement that the world was created in October. Columbus noted this, writing in the margin: "Bachon [sic] wrote to Pope Clement that without doubt the world was created in October." For a facsimile of the note, see de Lollis, *Autografi*, series C, plate 91, no. 821.

¹⁸ For the Latin text of chapter 8 of *Imago mundi* and Columbus's annotations on it, see Buron, *Imago mundi de Pierre d'Ailly*, 1: 206–15.

from Hispaniola in 1498 and the other from Dominica in 1503, Columbus asserted that his voyages had confirmed the cosmography of *Imago mundi* and the calculations of Alfraganus. In the latter of these two letters describing his fourth voyage, Columbus proclaimed:

The world is but small; out of seven divisions of it the dry part occupies six, and the seventh is entirely covered with water. Experience has shown it, and I have written it, with quotations from the Holy Scripture, in other letters, where I have treated of the location of the terrestrial paradise, as approved by the Holy Church; and I say that the world is not so large as vulgar opinion makes it, and that one degree from the equinoctial line measures fifty-six miles and two-thirds. This is a fact that one can touch with one's own fingers.¹⁹

Columbus's readings and his own testimony indicate that his image of the world was traditional rather than innovative. He did not believe that his Enterprise of the Indies would essentially alter a geography and cosmology that had existed since antiquity; he thought that his expedition would simply fill out that picture.

THE PREVAILING MODERN IMAGE OF COLUMBUS as intrepid sailor and enlightened geographer—the Admiral of the Ocean Sea—does not substantially differ from that established by Humboldt in the 1830s. This image continues to be based on the same slender repertoire of texts: the two letters from Toscanelli, the annotations on *Imago mundi*, and selections from Columbus's letters describing the four voyages of discovery. The works of well-known Columbus scholars, such as Harrisse, Cesare de Lollis, and J. N. Fiske in the nineteenth century and Vignaud and Morison in the twentieth, have not significantly revised the account of the genesis of Columbus's ideas given by Humboldt in the *Examen critique*. None of these scholars took seriously the possibility that Columbus's personal spirituality or the spirituality of his age might also have inspired him to undertake his voyages of discovery.²⁰

John Leddy Phelan—in his pioneering study, *The Millennial Kingdom of The Franciscans in the New World*, first published in 1956—was the first modern scholar to attempt to restore Christofereans to Columbus. In the second chapter of that book, "The Apocalypse in the Age of Discovery," Phelan emphasized the importance of contemporary apocalypticism, specifically Joachimism, to understanding the mentality of Columbus and his motivations for travel and discovery. More

¹⁹ R. H. Major, trans. and ed., *Christopher Columbus: Four Voyages to the New World* (New York, 1961), 177–78. Humboldt considered this citation to be important evidence of "l'impression profonde" that Columbus's reading of chapter 8 of *Imago mundi* had on his voyages of discovery; *Examen critique*, 61. On Columbus as a practical, as distinguished from a theoretical, navigator and geographer, see George E. Nunn, *The Geographical Conceptions of Columbus* (New York, 1977), 1–30. Nunn argued that Columbus used a degree that measured 56 2/3 Italian nautical miles, which was not equivalent to Alfraganus's figure, calculated on the basis of Arabian nautical miles. Columbus himself thought that he was navigating according to Alfraganus's figures. In a lengthy note to d'Ailly's *Epilogus mappae mundi*, he wrote: "Observe that in sailing often from Lisbon southward to Guinea, I carefully measured the course . . . and in agreement with Alfragan I found that each degree answered to 56 2/3 miles. So that we may rely upon this measure." J. N. Fiske, trans., *The Discovery of America*, 1 (Boston, 1983), 377–78. For the Latin original, see Buron, *Imago mundi de Pierre d'Ailly*, 2: 530–31; d'Ailly, *Incunabulum*, 42; and de Lollis, *Autografi*, series C, plate 82, no. 490.

²⁰ Vignaud's conclusion is representative of this prevailing point of view: "It is also necessary to mention that Las Casas and [Ferdinand] Columbus assigned a fourth source to the grand design of Columbus: divine inspiration. But that is an order of ideas that permits no critical discussion and one into which we need not enter here; it suffices to have mentioned it." *Histoire critique*, 8.

recently, well-known students of apocalypticism such as Morton Bloomfield and Bernard McGinn have pointed out the apparent importance of Joachimism and other apocalyptic movements in the age of discovery and have called for further study in this area.²¹ Only in the past several years have scholars begun to respond to these calls and to the argument that Phelan first sketched almost thirty years ago. In his *Colón y su mentalidad mesiánica en el ambiente franciscanista español*, published in 1983, Alain Milhou presented a much more detailed picture of Franciscanism in fifteenth-century Spain than has heretofore been available and followed Phelan in arguing that it is a principal source for Columbus's "messianic mentality." Although the historical background of Columbus's apocalypticism has become clearer with this study, the apparent paucity of sources that link Columbus's Enterprise of the Indies to contemporary apocalypticism or to the medieval prophetic tradition has remained a serious obstacle. But sources there are.

The incunabulum of d'Ailly's works that came into Columbus's hands was published sometime between 1480 and 1483 by John of Westphalia.²² In addition to *Imago mundi*, it contained a number of other opuscula by the Cardinal of Cambrai and by his contemporary Jean Gerson, the important nominalist and mystic. D'Ailly's opuscula, written in 1414, focused on the problem of the interrelationships between history, theology, astronomy, and prophecy. They included *Tractatus de legibus et sectis contra supersticiosos astronomos*, *Tractatus de concordia astronomice veritatis cum theologia* (known as the *Vigintiloquium*), *Tractatus de concordia astronomice veritatis et narrationis hystorice*, *Elucidarium astronomice concordie cum theologia et hystorica veritate*, *Apologetica defensio astronomice veritatis*, *Secunda apologetica defensio astronomice veritatis*, and *Tractatus de concordia discordantium astronomorum*. These opuscula involved a debate with Gerson over the legitimacy of astrology for forecasting the fulfillment of various biblical prophecies and the end of the world. Columbus read and annotated these short works by d'Ailly and Gerson as well as *Imago mundi*. He seems to have been considerably more interested, however, in d'Ailly's opuscula than in those of Gerson, with which this essay will not deal. Surprisingly, scholars have paid little attention to the content of d'Ailly's opuscula and to Columbus's annotations on them. What follows here is intended to fill in this lacuna, if only in a preliminary way.

Columbus intended to set forth his vision of history and the role that he was

²¹ Phelan, *The Millennial Kingdom of the Franciscans in the New World* (Berkeley and Los Angeles, 1970), 17–28; Bloomfield, "Recent Scholarship on Joachim of Fiore and His Influence," in Ann Williams, ed., *Prophecy and Millenarianism: Essays in Honour of Marjorie Reeves* (Essex, 1980), 37: "The need of investigations of Joachim in the New World is so obvious that it hardly needs saying"; and McGinn, *Visions of the End*, 284: "To those accustomed to seeing the discovery of America as the work of a hard-headed practical seaman flouting the traditions of the past, the picture of Columbus as a religious visionary strongly influenced by centuries of apocalyptic hopes may seem strange, but the existence of this element in the great explorer's personality is undeniable."

²² Buron believed that the incunabulum of d'Ailly's works was published in 1483 and pointed out that Humboldt and others merely repeated the date of 1490 that Jean de Launoy assigned to it. Salembier and de Lollis, refuting Humboldt, gave 1480 or 1483 as the date of publication. Lynn Thorndike used another copy of the same incunabulum that Columbus annotated and dated it "about 1480." Thorndike's discussion of d'Ailly's opuscula and the controversy with Gerson is the most extensive published to date. See Buron, *Imago mundi de Pierre d'Ailly*, 1: 29; Humboldt, *Examen critique*, 62; de Launoy, *Regii navarrae gymnasii parisiensis historia* (Paris, 1677), 477–78; Salembier, *Petrus de Albiaco*, xxiii; de Lollis, *Autografi*, vii; and Thorndike, *A History of Magic and Experimental Science*, 4 (New York, 1934), 101–31.

destined to play in it in a volume called the *Book of Prophecies*, which he worked on for a number of years, including the period of the last of his four voyages to the New World, but never completed. What survives is a collection of materials assembled by himself and a friend, the Carthusian monk Gaspar Gorricio, that Columbus apparently planned to incorporate into the *Book of Prophecies*. It consists of excerpts from the Bible and from a number of well-known ancient and medieval authors, some fragments of verse in Spanish (most likely not by Columbus), and an incomplete letter (written by Columbus sometime between September 13, 1501, and March 23, 1502, and addressed to Ferdinand and Isabella of Spain) that became the preface to the work.²³

De Lollis, who edited the *Book of Prophecies* in 1894, and more recently Phelan, among others, have stressed its importance for understanding Columbus. Because of its incomplete and composite nature, however, the *Book of Prophecies* has defied extensive analysis. This essay contends that it is Columbus's reading of d'Ailly's opuscula that provides the key to his plan for the *Book of Prophecies*. A plan, though latent, can be discerned in the pattern of sources selected by Columbus and Gorricio. Columbus discussed this latent plan more explicitly in the prefatory letter to the *Book of Prophecies* and in other letters describing his voyages to the New World. If the testimony in Columbus's marginalia and his letters is taken into account, these opuscula of d'Ailly played at least as important a role in motivating him to make his voyages of discovery as did his reading of *Imago mundi* and his alleged correspondence with Toscanelli.

When did Columbus read d'Ailly's opuscula? The question is vexing yet crucial for any argument regarding the genesis of the Enterprise of the Indies. It is well known that in a note at the beginning of chapter 8 of *Imago mundi* Columbus referred to a voyage made by Bartolomeu Dias in 1488, which makes that year a *terminus post quem*. But what has not been previously pointed out is a note in Columbus's hand to one of the other pieces in the incunabulum of d'Ailly's works,

²³ For a discussion of the text of the *Book of Prophecies*, see de Lollis, *Raccolta di documenti e studi pubblicati della R. Commissione Colombiana*, part 1, volume 2: *Scritti di Colombo* [hereafter, *Scritti*] (Rome, 1894), lvii–lx. De Lollis found the earliest mention of the *Book of Prophecies* in an inventory of items belonging to Columbus's son, "Admiral Don Diego." The *Book of Prophecies* is listed in a catalogue, compiled by Ferdinand, of Columbus's books, and Ferdinand mentioned it in the biography of his father, including a passage from the beginning of Columbus's letter to Ferdinand and Isabella contained in the *Book of Prophecies*. De Lollis said that this excerpt occurs in chapter 8 of Ferdinand's *Life of the Admiral Christopher Columbus*; it is in fact at the beginning of chapter 4. Also see Las Casas, *Historia de las Indias*, 47–48. In a note at the beginning of the script of the *Book of Prophecies*, Columbus wrote that, while resting from his third voyage to the New World (completed in October 1500), he decided to compile a series of excerpts illustrating "tragic events," which he hoped someday to put into verse, and asked Gorricio to gather other relevant passages. The letter is dated September 13, 1501; de Lollis, *Scritti*, 75. On March 23, 1502, when Columbus was preparing to depart for the fourth voyage, Gorricio sent some additional "jottings" that he had compiled. Columbus's letter to Ferdinand and Isabella intended as the preface to the *Book of Prophecies* can be dated between September 1501 and March 1502, that is, between the third and fourth voyages. Although the date of the prefatory letter has caused controversy, de Lollis believed that the letter is an autograph of Columbus; *Autografi*, xx–xxi. Four hands can be distinguished in the manuscript of the *Book of Prophecies*: Columbus's, Gorricio's, Ferdinand's, and that of Columbus's brother Bartolomeo; de Lollis, *Autografi*, xviii–xxii. De Lollis concluded that the work should be regarded essentially as Columbus's: "Let us repeat: considered in its purpose and its entirety, the *Book of Prophecies* serves marvelously to make sense out of the complex spirit of Christopher Columbus"; *Scritti*, lx. The manuscript of the *Book of Prophecies* is in the Biblioteca Colombina, Seville, Spain. For de Lollis's edited version, see *Scritti*, 75–160. He also published a facsimile of those parts of it that he considered to be in the hand of Columbus; *Autografi*, series F, plates 102–159.

De correctione kalendarii. In chapter 3 of that work, “De errore ex mutatione equinoxiorum et solstitiorum,” d’Ailly discussed the problem of accurately determining the date and time of the annual solstices and equinoxes. Using the Alphonsine tables (which Columbus also knew), d’Ailly made calculations for “this year 1411.” Columbus followed d’Ailly’s notations carefully, paraphrasing them in the margin. At the bottom of the page Columbus, using d’Ailly’s figures in *De correctione kalendarii* and others in *De legibus et sectis*, made his own calculation of when the vernal equinox would occur in “this year 1491.” On the basis of this note it can be argued that Columbus read *Imago mundi* and the other opuscula prior to his initial voyage of discovery in 1492.²⁴

AS ALREADY SHOWN, d’Ailly’s significance as a source for Columbus lies not in his originality as a thinker but in his diligence as a reader and digester of authoritative or influential works by other writers. In composing his opuscula on the interrelationships between history, astronomy, and theology, d’Ailly relied on a number of earlier medieval treatises that discussed the events to occur on the eve of the end of the world and that predicted when they would happen on the basis of planetary movements and conjunctions. Paramount among d’Ailly’s sources was book 4 of Roger Bacon’s *Opus maius*, especially sections on the application of mathematics to sacred subjects and on astrology. Not coincidentally, this was the same book containing Bacon’s discussion of geography, large sections of which d’Ailly incorporated into *Imago mundi*.²⁵

According to Bacon, the patriarchs and prophets discovered mathematics. Citing Josephus, Jerome, and Cassiodorus as his sources, he said that “the sons of Adam discovered geometry, astronomy, arithmetic, and music; and Noah and his sons taught the Chaldeans; then Abraham taught the Egyptians.” These ancients applied their knowledge of mathematics to a number of different sacred subjects, including the following: cosmography, which “stirs us to reverence the creator”; geography, “for the whole series of scripture deals with the regions, states, deserts, mountains, seas, and other places of the world”; and chronology, “for the whole course of history is traced through times and generations and ages from the beginning of the world to Christ the Lord, and all things have been set in order on his account, that no other legislator might be expected, but that he alone may be the saviour of the world by his own law.”²⁶

²⁴ For the text of Columbus’s note to chapter 8 of *Imago mundi*, see Buron, *Imago mundi de Pierre d’Ailly*, 1: 207; d’Ailly, *Incunabulum*, 13; and de Lollis, *Autografi*, series C, plate 70. For Columbus’s notes paraphrasing d’Ailly’s calculations for the solstices and equinoxes of 1411, see d’Ailly, *Incunabulum*, 60; and de Lollis, *Autografi*, series C, plate 90, nos. 619, 620. Columbus’s note to d’Ailly’s *De correctione kalendarii* mentioning “this year 1491” reads: “Note that in the calendar, the ascending of the solar year is ten minutes and forty-four seconds in any year, as is demonstrated at the end of the treatise on laws and sects [*De legibus et sectis*]. And it is confirmed that we can designate the vernal equinox of this year 1491 as the eleventh day of March at one hour, thirty-seven seconds, and forty-seven tierces past noon, accepting as a base that in the year 1411 the Sun entered the first point of Aries on the eleventh of March at fifteen hours, fifty-six minutes, seven seconds and forty-seven tierces after noon.” D’Ailly, *Incunabulum*, 60; and de Lollis, *Autografi*, series C, plate 90, no. 621.

²⁵ D’Ailly’s sources also included Albert the Great, Henry Bate, William of Auvergne, Vincent of Beauvais, Nicholas Oresme, and Henry of Hesse (Henry Langenstein). See Thorndike, *History of Magic*, 103–09.

²⁶ Robert Belle Burke, trans., *The Opus maius of Roger Bacon*, 1 (Philadelphia, 1928), 195–96. On the

The mathematical knowledge of the astronomers was essential to determining the configuration of the planets and hence the time of the year when the world began. From these determinations the dates of other important past events, such as the flood and the birth of Christ, and the time of future events prophesied in Scripture could be calculated. What Bacon particularly wanted to forecast was the coming of the Antichrist, which portends the imminent end of the world.²⁷

In his calculations Bacon relied on the theory of planetary conjunctions set forth by Albumasar, the ninth-century Arabian astronomer. Albumasar argued that the universe is a *sympatheia* in which the terrestrial and celestial worlds are bonded together in a reciprocally harmonious whole, governed by the movements of the planets and stars. Whoever comes to comprehend any part of the universe through the techniques of theurgy and astrology comprehends the whole and can predict the future, for all events are imprinted on the present. Important religious and historical events can be predicted as well as events in an individual's life. Following Albumasar's theory of planetary conjunctions, Bacon (and others) calculated the "horoscopes" of the great religions and empires in much the same way that one would cast a personal horoscope.²⁸

In *De legibus et sectis*, d'Ailly closely followed the theory of conjunctions set forth by Bacon in *Opus maius*. As he had done in *Imago mundi*, d'Ailly incorporated substantial sections of *Opus maius* directly into his own text. And, just as he had done with *Imago mundi*, Columbus carefully annotated *De legibus et sectis*, acquainting himself with Bacon's and d'Ailly's eschatology and theory of conjunctions. According to the ancient tradition expounded by Bacon and d'Ailly, Jupiter, one of two "benevolent and fortunate" planets, occupies the ninth house in the heavens, the "house of peregrinations and journeys of faith and deity and religion." Jupiter can enter into conjunction with six planets (including the sun and moon), each of which reigns over one of the great religions of the world. When one of these planets enters into conjunction with Jupiter, its religion becomes historically ascendant. The conjunction of Jupiter with Saturn signifies the ascendancy of the Jews, with Mars the Chaldeans, with the Sun the Egyptians, with Venus the Saracens, with Mercury the Christians, and with the moon the final sect of the Antichrist. Columbus followed this argument, presented by d'Ailly in chapters 1 and 2 of *De legibus et sectis*, noting that "there are six principal sects from the beginning of the

discovery of mathematics and the sacred subject of cosmology, see *ibid.*, 200–03. On geography, see *ibid.*, 203–08. On chronology, see *ibid.*, 208–32.

²⁷ Burke, *The Opus maius of Roger Bacon*, 276: "By the means offered by mathematics not only are we [theologians] made certain respecting our profession, but we are fortified in advance against the sect of the Antichrist, about which at the same time with the Church of Christ mathematics is concerned. A very excellent examination of this kind is made by considering all the principal sects from the beginning of the world. . . namely Jews, Chaldeans, Egyptians, Agareniens or Saracens, who descended from Agar and Ishmael, the Church of Christ, and the sect of the Antichrist. Nor is it strange if philosophers have spoken regarding these, since they were after the patriarchs and prophets and were instructed by their sons and books, as we have previously shown."

²⁸ Albumasar's major works, *Introductorium maius in astronomiam* and *De magnis coniunctionibus*, were translated into Latin in the twelfth century by John of Seville. Through these translations the so-called "theory of conjunctions" entered the Western tradition of cosmological speculation. See Richard J. Lemay, *Abu Ma'shar and Latin Aristotelianism in the Twelfth Century: The Recovery of Aristotle's Natural Philosophy through Arabic Astrology* (Beirut, 1962); Pierre Duhem, *Le système du monde: Histoire des doctrines cosmologiques de Platon à Copernic* (Paris, 1958), vol. 8, chap. 13; and Eugenio Garin, *Astrology in the Renaissance: The Zodiac of Life* (London, 1983), chap. 1.

world: the sects of the Hebrews, the Chaldeans, the Egyptians, the Saracens, Christ, and the Antichrist." "The conjunction of Jupiter with Saturn signifies the law of the Hebrews; if Jupiter is conjoined with Mars, it signifies the Chaldean law, which teaches the worship of fire; if Jupiter is conjoined with the Sun, it signifies the Egyptian law; if it is conjoined with Venus, it signifies the law of the Saracens; if it is conjoined with Mercury, the law of the Christians; if conjoined with the moon, the law of the Antichrist."²⁹

The point of ascendancy of each of these religions and the length of its stay in power is determined by the conjunctions of Jupiter and Saturn, which are of three kinds. As discussed by d'Ailly in chapter 4 of *De legibus et sectis*, they are as follows: a "great conjunction," which takes place every twenty years and refers to "the elevation of kings and potentates and to dearness in the cost of provisions, and to the rise of prophets"; a "greater conjunction," which takes place every 240 years and refers to "a sect and to its change in certain regions"; and a "greatest conjunction," which takes place every 960 years and refers to "changes in empires and kingdoms, to impressions of fire in the air, to flood, earthquakes, and dearness in the price of food." The fortunes of a particular sect may shift more quickly or slowly within this framework of great conjunctions depending on "the properties of the planets bearing sway over different regions": Saturn controls India; Jupiter, Babylonia; Mars, Thrace; the Sun, the Romans and their empire; Mercury, Egypt; the moon, Asia. Columbus absorbed this material as well, noting that "there are three kinds of conjunctions of Jupiter and Saturn, namely, a great conjunction, a greater conjunction and a greatest conjunction. . . . The greater conjunction takes place every 240 years . . . the greatest conjunction every 900 years," and "Saturn rules over India, Jupiter over Babylonia, Mars over Thrace, the Sun over Rome, Mercury over Egypt, the moon over Assyria."³⁰

Columbus's reading of the first four chapters of *De legibus et sectis* led him to a passage by d'Ailly copied directly from Bacon's *Opus maius*, which was apparently of such significance to him that he included it in the *Book of Prophecies*. Located in chapter 4 of *De legibus et sectis*, the passage concerns the length of time that the law of Muhammad (the law of the Saracens) will last and under what circumstances its demise might be predicted. D'Ailly (following Bacon) noted that "according to what Albumazar said, that law [of Muhammad] cannot last more than 693 years." Columbus marked this passage and copied it out verbatim in the margin. Albumasar, Bacon, and d'Ailly predicted that the followers of Muhammad, the Saracens, would be destroyed by either the Tatars or the Christians. Columbus also marked this passage, saying "the Saracens have already in large part been conquered by the Tatars and their capital, Baldac, and their caliph, 'who was like a Pope to them,' have been destroyed."³¹

²⁹ Burke, *The Opus maius of Roger Bacon*, 276–80. On Jupiter and its conjunctions with each of the other planets signifying the ascendancy of one of the major world religions, compare d'Ailly, *Incunabulum*, ff. 44v–45; and de Lollis, *Autografi*, series C, plate 83, nos. 513–18, 524.

³⁰ Burke, *The Opus maius of Roger Bacon*, 284–87; d'Ailly, *Incunabulum*, ff. 46v–47; and de Lollis, *Autografi*, series C, plate 83, nos. 535–37, 543.

³¹ For Bacon's text, see Burke, *The Opus maius of Roger Bacon*, 287. For a Latin edition, see John Henry Bridges, *The Opus maius of Roger Bacon*, 1 (London, 1900), 266. For the passage from d'Ailly's *De legibus et sectis*

In *Opus maius*, Bacon noted that these events had taken place only twelve years earlier. In *De legibus et sectis*, d'Ailly lamented the "big lapse since that time," during which the sect of the Tatars had not been finally destroyed. It would be useful for the church, he suggested, to determine when the final destruction of "that sect of perdition" was to come, since, according to "this doctor" [Bacon] and the "astronomers," "no sect comes after the law of Mohammed, only the Antichrist." Using a passage from *Opus maius*, d'Ailly predicted that the advent of this last sect was not far off:

Ethicus, the philosopher, says in his *Cosmography* that a race that has been shut up within the Caspian gates shall burst forth upon the world and meet the Antichrist and call him God of gods. This has already come true, just as he said: Have not the Tartars who were within those gates gone forth from them? For those gates have been broken as some Christians who travelled through the middle of them have returned. Therefore, he introduces this as a sign of the imminent advent of the Antichrist. In conclusion he says, "I know that, if the Church would be willing to unroll the sacred text and the holy prophecies of the Sybilline oracle and of Merlin, of Aquile and Joachim and many others, and besides the histories and the books of the philosophers, and if the Church were to order that the methods of astronomy be considered, it would discover what it needs to know, that is, some idea of greater certainty regarding the time of the Antichrist."³²

This passage was one of five excerpts from d'Ailly's opuscula that Columbus included in the *Book of Prophecies*.

It is significant that in their seminal article, "The Penetration of Joachimism into Northern Europe," Morton Bloomfield and Marjorie Reeves quoted the latter part of this passage (from Bacon's *Opus maius*) as indicative of the thirteenth-century sense "of crisis, of impending doom in history, that turned men's thought towards the Calabrian abbot." This "sense of crisis, of impending doom" still prevailed in the early fifteenth century when d'Ailly picked out the passage from Bacon for inclusion in *De legibus et sectis* and in the late fifteenth century when Columbus chose it for the *Book of Prophecies*. The *fortuna* of this passage from *Opus maius* is evidence of the perennial power of the apocalyptic vision of history derived from the prophetic tradition, regardless of shifting historical circumstances.³³ The extent to which it can be specifically or exclusively associated with Joachimism is problematic. Of this more below.

A second important passage that Columbus marked for the *Book of Prophecies* is located in another of the opuscula, *Tractatus de concordia astronomice veritatis et narrationis hystorice*, which consists of eight preambles, future events that will presage the appearance of the Antichrist. In the left-hand margin at the beginning of the

included in the *Book of Prophecies*, see d'Ailly, *Incunabulum*, f. 47v. For the text edited by de Lollis, see *Scritti*, 105–06. For facsimiles of the two marginal annotations by Columbus, see de Lollis, *Autografi*, series C, plate 83, nos. 544, 545.

³² D'Ailly, *De legibus et sectis*, chap. 4, in d'Ailly, *Incunabulum*, f. 47v; and de Lollis, *Scritti*, 106: "But not withstanding these matters, and though much time has passed since then, yet experience teaches that the ruinous sect has not yet been destroyed, for, alas, it has often strongly prevailed against the Christians." For the passage that d'Ailly copied from Bacon, which has been slightly edited here, see Burke, *The Opus maius of Roger Bacon*, 289–90. For the Latin text, see Bridges, *The Opus maius of Roger Bacon*, 268–69. For the transcription in the *Book of Prophecies*, see de Lollis, *Scritti*, 106–07.

³³ Bloomfield and Reeves, "The Penetration of Joachimism into Northern Europe," *Speculum*, 29 (1954): 772–93. The passage quoted is on page 786.

passage, Columbus drew a hand whose index finger pointed to the text. He used this common medieval device throughout the incunabulum of d'Ailly's works to mark passages that apparently were of particular significance to him. Accompanying this indicator is a notation in Columbus's hand, "Jerome mentions that Methodius the martyr writes many things concerning the completion of the ages," a reference to Jerome's brief entry on Methodius in chapter 83 of his *Liber de viris illustribus*. The entire passage is marked by a line drawn parallel to it.³⁴

D'Ailly's eight preambles for the advent of the Antichrist are taken directly from chapters 10–13 of the pseudo-Methodius's *Sermo de regno cantium et in novissimis temporibus certa demonstratio*, one of the most influential apocalyptic texts among the many that circulated throughout the Middle Ages and Renaissance. The section that d'Ailly lifted and Columbus took for the *Book of Prophecies* is one of the best-known parts of that well-known work.³⁵

The first preamble describes the coming of the "son of perdition" as predicted by Paul in the second letter to the Thessalonians: "The day of Christ is at hand. Let no man deceive you by any means: for that day shall not come except there come a falling away first, and that man of sin be revealed, the son of perdition; who opposes and exalts himself above all that is called God."³⁶ Paul's "son of perdition" was widely believed in the Middle Ages to be the Antichrist. The second preamble predicts the uprising of the sons of Ishmael, of whom Daniel spoke, against the Roman empire. This will take place in the seventh millennium, on the eve of the end of time. During this millennium, according to d'Ailly's third preamble, the sons of Ishmael, whom he identified with the Saracens, will overrun the promised land of the Christians, which Paul predicted in 2 Thessalonians. On account of their sins, the inhabitants of the promised land will fall victim to other, more horrible perversions—for example, sodomy. The fourth preamble further discusses this period of dissension and torment, saying that the spirit of the faithful will be much diminished and that many will abandon their faith altogether. Preambles 3 and 4 paraphrase chapters 11 and 12 of the pseudo-Methodius's sermon on the end of the world.³⁷

According to d'Ailly's fifth preamble, the sons of Ishmael will celebrate the desolation that they have wrought throughout the world, saying that the Christians will never escape their domination. But suddenly an emperor-messiah, "the king of the Romans," will appear from the "Ethiopian Sea in a great fire" and will conquer the sons of Ishmael, imposing "his yoke upon them seven times as much as their

³⁴ D'Ailly, *Tractatus de concordia astronomice veritatis et narrationis hystorice*, chapter 61, in d'Ailly, *Incunabulum*, ff. 120–120v; and de Lollis, *Scritti*, 108–09. For a facsimile of Columbus's annotation, see de Lollis, *Autografi*, series C, plate 91, no. 795.

³⁵ I used the edition of the pseudo-Methodius's sermon contained in Ernst Sackur's *Sybillinische Texte und Forschungen: Pseudo-Methodius, Adso und die Triburtinische Sibylle* (Halle, 1898). On the place of the pseudo-Methodius in the prophetic tradition, see Paul J. Alexander, "Byzantium and the Migration of Literary Works and Motifs: The Legend of the Last Roman Emperor," *Medievalia et Humanistica*, new ser., 2 (1971): 47–82; Reeves, *Influence of Prophecy*, pt. 3; and McGinn, *Visions of the End*, pt. 1, chap. 7.

³⁶ 2 Thess. 2:2; and Sackur, *Sybillinische Texte und Forschungen*, 78.

³⁷ For the second preamble, see d'Ailly, *Incunabulum*, f. 120; and de Lollis, *Scritti*, 108–09. For chapter 10 of the pseudo-Methodius, see Sackur, *Sybillinische Texte und Forschungen*, 80. For the third and fourth preambles, see d'Ailly, *Incunabulum*, f. 120; and de Lollis, *Scritti*, 109. For chapters 11 and 12 of the pseudo-Methodius, see Sackur, *Sybillinische Texte und Forschungen*, 80–88.

yoke weighed upon the earth." The sixth preamble states that, after the fiery revenge of the "king of the Romans," "there will be a time of great peace and tranquillity upon the earth such as there has never been, for there is nothing like that final peace that is at the end of time." This is the peace of which Paul spoke in the second letter to the Thessalonians. Both of these preambles closely follow the contents of chapter 13 of the pseudo-Methodius and the sequence of his presentation.³⁸

D'Ailly's seventh preamble follows the latter part of the pseudo-Methodius's chapter 13. It predicts, after this period of profound tranquillity, another period of great turmoil during which the forces of Gog and Magog will be unleashed. "The gates of the north will be opened and the strength of those nations which Alexander enclosed within them will go forth." The "enclosed nations" will corrupt the earth for "a week of years," whereupon a divinely sent warrior prince will conquer them in an instant. The eighth preamble, directly quoting the conclusion of the pseudo-Methodius's chapter 13, completes the sequence of events that will precede the coming of the Antichrist. Following the defeat of the "enclosed nations," the "king of the Romans" will live in Jerusalem for ten and one-half years, at which time the "son of perdition" will appear.³⁹

D'Ailly's opuscula also provided Columbus with a larger chronological framework within which to place the rise and decline of the great religions of the world, the defeat of the Saracens by the Christians, and the events leading to the appearance of the Antichrist. In *Vigintiloquium*, d'Ailly discussed the duration of the world. In verbum 11 of that work, he took Augustine as his authority, specifically citing book 22, chapter 30, of *The City of God* and Augustine's sermon on the Sixth Psalm. D'Ailly may himself have selected these texts. The passage from *The City of God*, a succinct summary of the Augustinian periodization of history, was again well known and influential throughout the medieval and Renaissance periods. In it Augustine listed seven ages corresponding to the number of days in the week of creation:

The first age, corresponding to the first day, is from Adam to the flood, the second from then on til Abraham. These are equal, not in years, but in the number of generations, for each age is found to have ten. From this point, as the evangelist Matthew marks off the periods, three ages follow, reaching to the coming of Christ, each of which is completed in fourteen generations: one from Abraham to David, the second from then until the deportation to Babylon, the third from then until the birth of Christ in the flesh. Thus there are five ages in all. The sixth is now in progress and is not to be measured by any fixed number of generations, for the scripture says: "It is not for you to know the times which the Father has fixed by his own power" (Acts 1:17). After this age God will rest, as on the seventh day, when he will cause the seventh day, that is, us, to rest in God himself. . . . This will be our sabbath and its end will not be an evening, but the Lord's day, an eighth eternal day, sanctified by the resurrection of Christ, which prefigures the eternal rest of both spirit and body.⁴⁰

³⁸ For the fifth and sixth preambles, see d'Ailly, *Incunabulum*, f. 120v; and de Lollis, *Scritti*, 109. Also see Sackur, *Sibyllinische Texte und Forschungen*, 89–91.

³⁹ For the seventh and eighth preambles, see d'Ailly, *Incunabulum*, f. 120v; and de Lollis, *Scritti*, 109. Also see Sackur, *Sibyllinische Texte und Forschungen*, 91–93.

⁴⁰ William M. Green, trans., *Saint Augustine: The City of God against the Pagans*, 7 (Cambridge, Mass., 1972),

D'Ailly also noted that Augustine in his sermon on the Sixth Psalm said that the world would last for seven thousand years, a millennium for each of the seven days in the week of creation. Columbus followed this presentation, noting in the margin next to d'Ailly's text Augustine's reference in the sermon on the Sixth Psalm to "those who believed that there were seven thousand years from Adam to the coming of the Lord to judge." He included the passage from *The City of God* cited above in the *Book of Prophecies*.⁴¹

THESE EXAMPLES SUGGEST THAT D'AILLY (and through him Bacon, the pseudo-Methodius, Augustine, and others) must now be considered a principal source of Columbus's apocalypticism and of his "scientific" geographical knowledge. Put another way, d'Ailly in *Imago mundi* and in the opuscula provided Columbus with summaries of contemporary knowledge about the physical cosmos and about that cosmos's eschatology. It was a cosmos with a mathematically determinable beginning and end, unfolding in time and space according to divine plan. Certain events forecast by the prophets would have to take place in order for that plan to be completed. Through proper application of the mathematical arts, discovered by the patriarchs and prophets, to the sacred subjects of cosmology, geography, and chronology, men could understand the past, present, and future of this divine plan. In the *Book of Prophecies* Columbus intended to set forth that eschatology and to explain his role in it.

Set against the background provided by d'Ailly, the selections from the Psalms and the Old Testament prophets that form a large part of the texts assembled for inclusion in the *Book of Prophecies* begin to manifest certain themes. Two themes emerge consistently in the selections from the Psalms. The first is a virtual obsession with the recovery of Mount Zion, symbol of the Holy Land. A typical example is the following citation from Psalm 2:6–8:

I indeed have anointed my king
On Zion, my holy hill.
Let me tell of the decree of the Lord:
He said to me, "you are my son;
Today I have begotten you.
Ask of me and I will make the
nations your inheritance,
and the ends of the earth your possession.

The second theme that runs through the selections from the Psalms is the conquering and conversion of the heathen, as, for example, in Psalm 18:43–44:

Thou dost establish me as the head of the
nations;
People that I have not known
serve me;
As soon as they hear of me they submit to me.

383–85. The passage from Augustine's sermons on the Psalms to which d'Ailly referred may be found in *Sancti Aurelii Augustini Enarrationes in Psalmos*, in Jacques Paul Migne, ed., *Patrologia Latina*, vol. 36, col. 90.

⁴¹ For the text of the passage from d'Ailly's *Vigintiloquium* included in the *Book of Prophecies*, see de Lollis, *Scritti*, 107. For a facsimile of Columbus's annotation, see de Lollis, *Autografi*, series C, plate 88, no. 716.

Another example is Psalm 22:27–28:

All the ends of the earth will remember
and turn unto the Lord;
And the clans of the nations will worship
before him.
For the kingdom belongs to the Lord;
And he rules over the nations.

Many other similar excerpts from the Psalms compiled in the *Book of Prophecies* could be cited.⁴²

The selections from the prophets collected in the *Book of Prophecies* also center on predictions concerning the recovery of Mount Zion by the faithful and the demise of the enemies of God. But there are others foretelling the triumph of Jerusalem following a period of world-wide conflict and the unleashing of Gog and Magog. These events are described in considerable detail in chapters 11 and 12 of the Book of Daniel and chapters 38 and 39 of Ezekiel, all cited in the *Book of Prophecies*. But a briefer and more general prediction from Isaiah, to whom Columbus accorded special status as a prophet in the prefatory letter to the *Book of Prophecies*, is the best example:

For the Lord will have mercy on Jacob, and will yet choose Israel, and set them in their own land: and the strangers shall be joined with them, and they shall cleave to the house of Jacob. And the people shall take them, and bring them to their place: and the house of Israel shall possess them in the land of the Lord for servants and handmaidens: and they shall take them captives, whose captives they were; and they shall rule over their oppressors (Isa. 14:1–2).

As in the case of the Psalms, many similar examples drawn from other prophets were collected in the *Book of Prophecies*.⁴³

Columbus's preoccupation with the final conversion of all races on the eve of the end of the world is also reflected in the particular attention that he paid to John 10:16: "And other sheep I have, which are not of this fold: them also I must bring, and they shall hear my voice; and there shall be one fold, and one shepherd." This famous verse, which lay at the heart of so many medieval and Renaissance apocalyptic texts, was quoted in its entirety in the *Book of Prophecies* along with glosses from Augustine, John Chrysostom, Gregory the Great, and Nicholas of Lyra.⁴⁴

The *Book of Prophecies* concludes with what at first seems to be a baffling collection of biblical passages in which the islands of Tarshish, Cathyr, and Ophyr are mentioned. Columbus's fascination with islands might be part of what Leonardo Olschki, the great Italian scholar, has called the medieval "romanticismo insulare" stemming from Marco Polo's *Il milione* and other popular travel literature. It seems

⁴² On the recovery of Mount Zion by the faithful, see, for example, Pss. 2:6–8, 48:1–2, 79:1, 50:1–2, 102:13, 122:1–2. On the final conquest and conversion of the heathen, see Pss. 18:43–44, 22:27–28, 46:10, 57:9–11, 67:3–4, 72:10–11, 86:8–10, 96:1, 106:47, 115:1–6.

⁴³ For other examples, see Isa. 14:1, 25, 35:10, 24:23, 41; Jer. 4:31, Bar. 5, Dan. 11–12, Ezek. 38–39, Joel 2, Amos 9, Micah 4, 5, 6. A greater part of the book of Isaiah is included in the *Book of Prophecies*, which is probably not coincidental. Phelan mentioned that Isaiah was one of the most popular Old Testament prophets among the Joachimites; *Millennial Kingdom*, 135 n. 25. Reeves noted that Joachim in *Liber concordiae* made Isaiah the symbol of the second status—the *ordo clericorum*; *Influence of Prophecy*, 18.

⁴⁴ De Lollis, *Scritti*, 105.

more plausible, however, that this fascination is an aspect of his more general interest in the *unum ovile et unus pastor* theme. A passage from Augustine's *De divinatione daemonum* included in the *Book of Prophecies* supports this theory:

God will prevail, it is said, against them and he will wipe out all the gods of the peoples of the earth, and they will adore him, each one from its own place, all the peoples of the islands. And indeed not only the peoples of the islands, but all peoples, so that elsewhere he does not name all the peoples of the islands, but the universal orb of the earth, saying: the universal ends of the earth will remember God and be converted to him and the people of the earth will adore his fatherly aspect since God is king and rules over the people.⁴⁵

Even the most remote, undiscovered islands will be converted to Christianity before the world ends. In the prefatory letter to the *Book of Prophecies* Columbus affirmed his certainty that his own discoveries—*islands he thought to be part of the oriental archipelago described by Marco Polo*—had greatly accelerated this final process of world-wide conversion.

Against this backdrop of material relating to the universal eschatology that Columbus intended to unfold in the *Book of Prophecies* may be placed a number of selections that appear to be part of the personal role he meant to establish for himself in this drama. A prophetic passage taken from *Soliloquiorum animae ad Deum*, a popular devotional work attributed to St. Augustine, evokes the inevitability of personal destiny in a general sense. "Before you formed me in the belly, you knew me, and before I left the womb, whatever pleased you was preordained for me. And those things that concerned me were written in your book, in the secret of your counsel."⁴⁶ Two others make more specific predictions. Not coincidentally one evokes the achievement of the Admiral of the Ocean Sea and the other, that of Christoferens. The first passage, one of a number of entries under the general heading "de presenti et futuro," is taken from Seneca's tragedy *Medea* (376): "The years will come, in the succession of the ages, when the Ocean will loose the bonds by which we have been confined, when an immense land shall lie revealed, and Tethys shall disclose new worlds, and Thule will no longer be the most remote of countries." This passage is followed by its paraphrase in Spanish. Written next to it in the hand of Ferdinand is the sentence, "My father, the Admiral Christopher Columbus, fulfilled this prophecy in the year 1492."⁴⁷

The second prophecy that can be applied to Columbus is attributed by him to Joachim of Fiore. It predicts that "he who will restore the ark of Zion will come from Spain." Neither Phelan nor Reeves could identify this prophecy, though

⁴⁵ *Ibid.* Also see *De divinatione daemonum*, in Migne, *Patrologia Latina*, vol. 36, col. 584. For Olschki's discussion, see his *Storia letteraria*, chap. 2, sect. 3, 38–55.

⁴⁶ This passage is not from Augustine's *Soliloquies* but from *Soliloquiorum animae ad Deum*, one of a triumvirate of popular devotional works widely attributed to him (the other two were known as the *Liber meditationum* and *Manuale*). Marcel Bataillon concluded that these works (translated into Spanish) were extremely widespread and influential in early sixteenth-century Spain, that is, during the period in which Columbus and Gorricio were compiling the *Book of Prophecies*; Bataillon, *Erasmus y España: Estudios sobre la historia espiritual del siglo XVI* (2d edn., Mexico, 1966), 47. For the Latin text of the passage quoted from the *Book of Prophecies*, see de Lollis, *Scritti*, 97. Also see Migne, *Patrologia Latina*, vol. 40, col. 884; and Jer. 1:5.

⁴⁷ De Lollis, *Scritti*, 141. Ferdinand also cited this passage from the *Medea* in chapter 8 of his biography and added, "Now it is considered certain that this prophecy is fulfilled in the person of the Admiral"; *Life of the Admiral Christopher Columbus*, 18.

Phelan believed that it stemmed from Arnold of Villanova (ca. 1250–1312), a Joachite and diplomat for the brothers James II of Aragon and Frederick III of Sicily. Phelan's guess was correct. In fact, José Pou y Martí had already published (in 1940), in his *Visionarios, beguinos y fraticelos catalanes (siglos XIII–XV)*, the section of Arnold's *De cymbalis ecclesiae* containing the prophecy attributed to Joachim. Pou y Martí's materials and themes were such that he had no reason to make the connection with Columbus, and he did not. But Milhou made the connection in his *Colón y su mentalidad mesiánica*. He has now unveiled the origins of the *Ve mundo* prophecy and its complicated medieval *fortuna*. The *Ve mundo* prophecy circulated in Aragon from Arnold's lifetime onward, being interpreted in various ways according to changing political circumstances. The version of the *Ve mundo* prophecy that Columbus knew was contained in a letter apparently addressed to Ferdinand and Isabella by Genoese legates in 1492 on the occasion of the Spanish capture of Granada from the Muslims. This event was interpreted by Columbus and others as evidence of the key role to be played by the house of Aragon in restoring the "ark of Zion" to the faithful—that is, the recapture of Jerusalem from the infidel. The letter from the Genoese delegates containing the *Ve mundo* prophecy attributed to Joachim was thus probably an example of the curious medieval mixture of political propaganda and eschatology discussed by Milhou.⁴⁸

THE THEMES THAT HAVE EMERGED in this examination of the materials that Columbus and Gorricio gathered for the *Book of Prophecies*—the recovery of the Holy Land and the final conversion of all peoples of the world set against the universal eschatology provided by d'Ailly's opuscula—were pulled together by Columbus in the prefatory letter addressed to Ferdinand and Isabella of Spain. Somewhat rambling and incomplete, this letter is nonetheless essential for understanding Columbus's self-image as the Christ-bearer.

Columbus began this letter by recalling his lifelong experience as a navigator and mapmaker and his many encounters with learned men:

When I was very young I went to sea to sail and I continue to do it today. This art predisposes one who follows it towards the desire to know the secrets of the world. More than forty years have already passed in which I have engaged in this activity; I have gone to every place that has heretofore been navigated. I have dealt with and held conversations with learned men, ecclesiastics and secular, Latins and Greeks, Jews and Moors, and with many others of other sects. I have found Our Lord very well disposed towards my desire, and I have from him the spirit of intelligence for carrying it out. He has bestowed the marine arts upon me in abundance and that which is necessary to me from astrology, geometry, and arithmetic. He has given me adequate inventiveness in my soul and hands capable of drawing spheres and situating upon them the towns, the rivers, mountains, islands, and ports, each in its proper place.⁴⁹

⁴⁸ For the text of the prophecy as it appeared in the *Book of Prophecies*, see de Lollis, *Scritti*, 148. For Phelan's discussion of it, see *Millennial Kingdom*, 135–36, and n. 27. Also see Reeves, *Influence of Prophecy*, 316–17. For the text of the *Ve mundo* prophecy contained in Arnold of Villanova's *De cymbalis ecclesiae*, see José Pou y Martí, *Visionarios, beguinos, y fraticelos catalanes (siglos XIII–XV)* (Vich, 1930), 54–55. For Milhou's discussion of the *Ve mundo* prophecy, see *Colón y su mentalidad mesiánica*, 375–483. For the role that Spain and its rulers were destined to play, see Phelan, *Millennial Kingdom*, chap. 1.

⁴⁹ De Lollis, *Scritti*, 79.

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But he explicitly denied that these activities and encounters were what ultimately caused him to succeed in the Enterprise of the Indies. Nor, in fact, did he believe that this enterprise was his final goal:

I spent six years here at your royal court, disputing the case with so many people of great authority, learned in all the arts. And finally they concluded that it all was in vain, and they lost interest. In spite of that it [the voyage to the Indies] later came to pass as Jesus Christ our Saviour had predicted and as he had previously announced through the mouths of His holy prophets. Therefore, it is to be believed that the same will hold true for this other matter [the voyage to the Holy Sepulchre]. If what I myself say does not seem to be sufficient evidence of this, I offer that of the Holy Gospel, which says that everything shall pass save for His marvelous Word. And in saying that, it says that everything must come to pass as it has been written by Him and by the prophets. . . . *I have already said that reason, mathematics, and mappamundi were of no use to me in the execution of the enterprise of the Indies.* What Isaiah said was completely fulfilled and that is what I wish to write here in order to remind Your Highnesses of it so that you may rejoice when I tell you by virtue of the same authorities that you are assured of certain victory in the enterprise of Jerusalem if you have faith.⁵⁰

It was divine inspiration that Columbus believed had come to him—and to Ferdinand and Isabella—and that urged him on to the final goal—the reconquest of the Holy Land:

Who would doubt that this light, which comforted me with its rays of marvelous clarity . . . and urged me onward with great haste continuously without a moment's pause, came to you in a most deep manner, as it did to me?

In this voyage to the Indies Our Lord wished to perform a very evident miracle in order to console me and the others in the matter of this other voyage to the Holy Sepulchre.⁵¹

And it was the opuscula of d'Ailly, not the "Joachimism" that Phelan suggested but was unable to substantiate, that provided Columbus with the eschatological framework within which to place his Enterprise of the Indies and what he conceived as his final mission.

In the prefatory letter he made this clear, drawing on the references to Augustine and d'Ailly that he had gathered for the *Book of Prophecies* and calculating that the end of the world was but 155 years away:

St. Augustine says that the end of this world will come in the seventh millennium from its creation; the holy theologians follow him, especially the Cardinal Pierre D'Ailly in Chapter XI of his *Vigintiloquium* and in other places, as I will mention below.

From the creation of the world, or from Adam, until the coming of Our Lord Jesus Christ are five thousand three hundred and forty-three years and three hundred and eighteen days according to the reckoning of King Alfonso, a reckoning that is held to be the most certain. Pierre D'Ailly, in Chapter X of his *Elucidario astronomice concordie cum theologia et hystorica veritate*, adds to it a little under one thousand five hundred and one years to make altogether a little under six thousand eight hundred and forty-five years.

According to this calculation, there are lacking about one hundred and fifty-five years for the completion of the seven thousand at which time the world will come to an end, as is said in the authorities mentioned above.⁵²

⁵⁰ *Ibid.*, 80, and 82 [my emphasis].

⁵¹ *Ibid.*, 79–80.

⁵² *Ibid.*, 81. A single sentence taken from d'Ailly's *Elucidarium astronomice concordie cum theologia et hystorica*

Columbus went on to refer his monarchs to the last nine chapters of d'Ailly's *Tractatus de concordia astronomice veritatis et narrationis hystorice* for details regarding the coming of the Antichrist. These nine chapters include chapter 63, discussed above, in which d'Ailly presented the eight preambles for the coming of the Antichrist. All of the preambles blended Bacon's theory of planetary conjunctions with the eschatology of the pseudo-Methodius. D'Ailly considered Joachim to have been an important prophet and mentioned him frequently throughout the opuscula, more often than not in a distinguished list that included Hildegard, Merlin, and the Sybilline prophet. But he nowhere discussed Joachim's eschatology in any detail as he did the eschatologies of Bacon and the pseudo-Methodius. So, if Columbus was indeed a "Joachimite," we do not know where he derived his knowledge of Joachim's theory of history and how it figured in his plan for the *Book of Prophecies*. On the other hand, he did read d'Ailly's opuscula, and the evidence suggests that he intended to incorporate the eschatology set forth in them into his own work.⁵³

This eschatological framework was probably already in place when Columbus made his initial voyage of discovery in 1492. In the letter to Ferdinand and Isabella that prefaces the journals of the first voyage and describes events leading up to that voyage, Columbus evoked the intermingled destinies of himself and his monarchs:

Most Christian and most exalted and most excellent and most mighty princes, King and Queen of the Spains and of the islands of the sea, our Sovereigns: . . . in this present year of 1492, after Your Highnesses made an end of the war with the Moors who reigned in Europe, and had brought that war to a conclusion in the very great city of Granada . . . in that same month, on the ground of information which I had given Your Highnesses concerning the lands of India, and concerning a prince who is called "Grand Khan," [and concerning] how many times he and his ancestors had sent to Rome to beg for men learned in our holy faith, in order that they might instruct him therein, and how the Holy Father had never made provision in this matter, and how so many nations had been lost, falling into idolatries and taking to themselves doctrines of perdition, Your Highnesses, as Catholic Christians and as princes devoted to the holy Christian faith and propagators thereof, and enemies of the sect of Mahomet and of all idolatries and heresies, took thought to send me, Christopher Columbus, to the said parts of India, to see those princes and peoples and lands and the character of them and of all else, and the manner which should be used to bring about their

narratione for the *Book of Prophecies* reads: "From the creation of Adam to the time of Christ, according to Alfonso, there are five thousand, three hundred and forty three years and three hundred and eight days"; de Lollis, *Scritti*, 107. Columbus closely followed verbum 9 of d'Ailly's *Elucidarium*, where d'Ailly discussed the duration of the six ages of the world; *Incunabulum*, ff. 125–26. Columbus annotated it copiously. See de Lollis, *Autografi*, series C, plate 91, nos. 800–07. At the conclusion of d'Ailly's discussion (d'Ailly, *Incunabulum*, f. 126) Columbus noted, "From the creation of the world up to Christ, according to Alfonso, there are 5,326 years, 3 days, 16 hours, and thirty minutes of the hour, according to one calculation; and according to another, otherwise"; de Lollis, *Autografi*, series C, plate 91, no. 807.

⁵³ The last nine chapters of *Tractatus de concordia astronomice veritatis et narrationis hystorice* are as follows: "De aliis decem revolutionibus saturnalibus et postea gestis"; "De his que ante complementum decem aliarum revolutionum gesta sunt"; "De scismatibus ecclesie"; "De magnis ecclesie scismate"; "De octava coniunctione maxima"; "De adventu antichristi et eius secta"; "De sex sectis principalibus secundum astronomos"; "De octo preambulis adventus antichristi secundum methodium"; "De antichristi ortu et fine et de consummatione seculi secundum eundem." On Columbus's "Joachimism," see Phelan, *Millennial Kingdom*, 22, and nn. 25, 26. There is evidence that in his later years Columbus was associated with the Franciscan Tertiaries—the branch of the order open to laymen. According to Las Casas, Columbus appeared in public in Seville dressed in the robes of a Franciscan; *Historia de las Indias*, 89. Columbus's son Diego related that his father was buried in the robes of a Franciscan Tertiary; Phelan, *Millennial Kingdom*, 19, and nn. 6, 7.

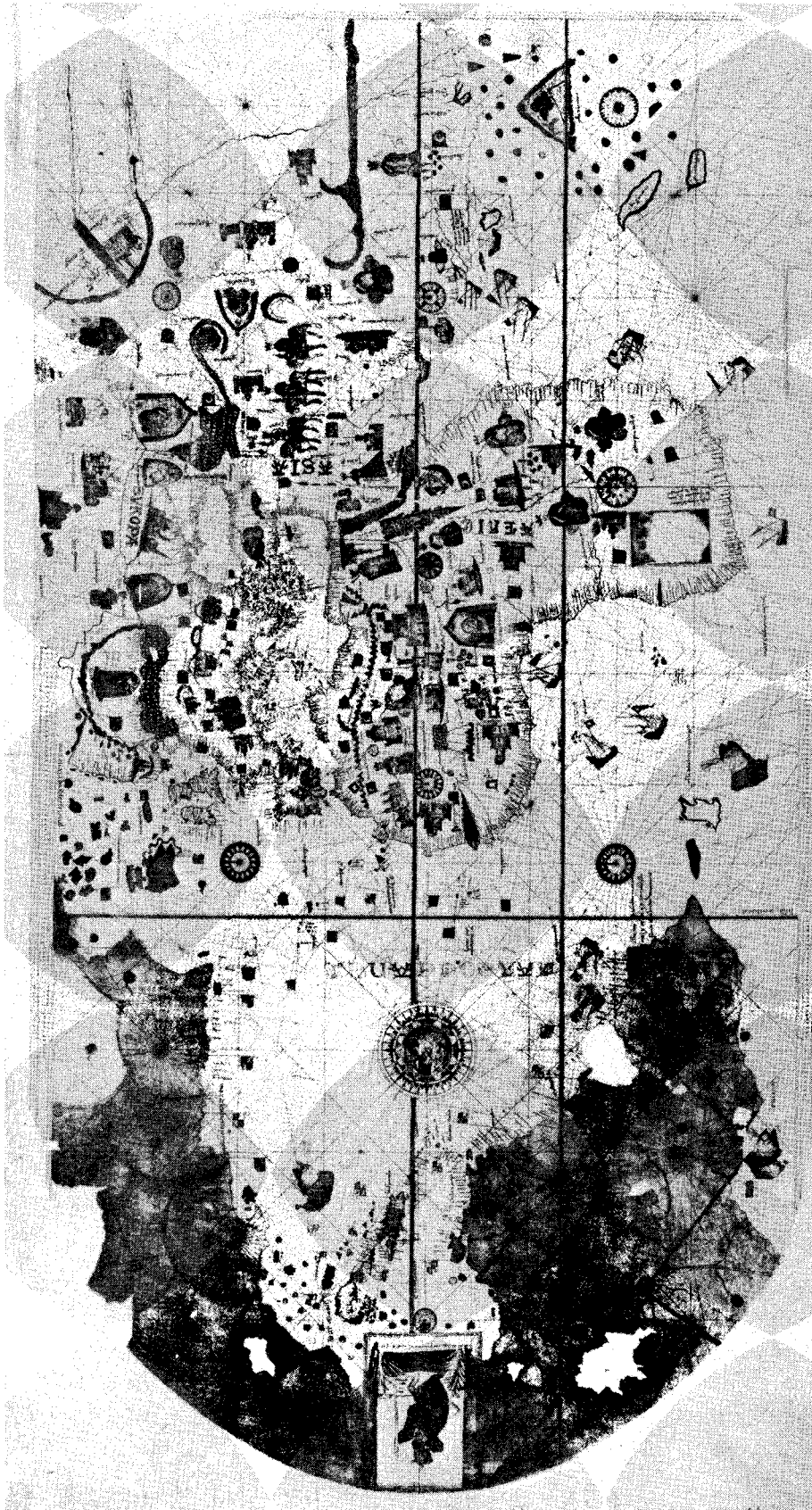


Figure 3: The 1500-05 map of Juan de la Cosa. Reproduction made from a nineteenth-century photolithograph in the collection of the William L. Clements Library, Ann Arbor, Mich. Photograph reproduced courtesy of John Dann, director of the Clements Library.

conversion to our holy faith, and ordained that I should not go by land to the eastward, by which way it was the custom to go, but by way of the west, by which down to this day we do not know certainly that anyone has passed.⁵⁴

Columbus's sentiments echoed those of many of his contemporaries who were convinced that, with the victory over the Moors at Granada, the time when the Spanish monarchy would play a special role in history had arrived. Phelan demonstrated how the ancient emperor-messiah myth, which was an essential part of medieval apocalypticism, was applied to the Spanish monarchy by early Franciscan missionaries such as Gerónimo de Mendieta.⁵⁵ And Marcel Bataillon and Milhou have written about the more general revival of Joachimism and other forms of apocalypticism, particularly among the Franciscans and their Tertiaries, in fifteenth- and sixteenth-century Spain. Events such as the conquest of Granada and the discovery of the Americas fired contemporary expectations that the recovery of Jerusalem from the infidel and the final conversion of all races prophesied in the Apocalypse would soon come to pass.⁵⁶

Columbus clearly shared these expectations. In his letter to Ferdinand and Isabella describing the fourth voyage of discovery, Columbus offered his services for one last journey:

Jerusalem and Mount Zion are to be rebuilt by the hands of the Christians as God has declared by the mouth of his prophet in the fourteenth Psalm (vv. 7–8). The Abbé Joaquin said that he who should do this was to come from Spain; Saint Jerome showed the holy woman the way to accomplish it; and the Emperor of China has, some time since, sent for wise men to instruct him in the faith of Christ. Who will offer himself for this work? Should anyone do so, I pledge myself, in the name of God, to convey him safely thither, provided the Lord permits me to return to Spain.⁵⁷

This was Columbus's ultimate goal, the purpose of all his travels and discoveries—the liberation of the Holy Land.

IN THE FINAL YEARS OF HIS LIFE, with his discovery of the unknown islands of the Indies, Columbus came increasingly to see himself as a divinely inspired fulfiller of prophecy, the one who inaugurated the age of the *unum ovile et unus pastor* forecast by John the Evangelist and John of Patmos. Columbus's vision of himself as the Christ-bearer was depicted by one Juan de la Cosa, who made what is generally considered to be the earliest map of the New World (see Figure 3).⁵⁸ Historians of

⁵⁴ Cecil Jane, trans., *The Journal of Christopher Columbus* (London, 1960), 3–4.

⁵⁵ Phelan, *Millennial Kingdom*, 11. On the emperor-messiah myth, see Reeves, *Influence of Prophecy*, pt. 3, "Joachimist Influences on the Idea of a Last World Emperor," *Traditio*, 17 (1961): 323–70; Franz Kampers, *Die deutsche Kaiser-idee in Prophetie und Sage* (Munich, 1896); and R. Folz, *Le souvenir et la légende de Charlemagne*, (Paris, 1950).

⁵⁶ Bataillon, *Erasmus y España*, 51–61, "Évangélisme et millénarisme au Nouveau Monde," *Courants religieux et humanisme à la fin du XV^e et au début du XVI^e siècle* (Paris, 1959), 25–36; Robert Ricard, *The Spiritual Conquest of Mexico: An Essay on the Apostolate and the Evangelizing Methods of the Mendicant Orders in New Spain, 1523–1572*, trans. Lesley Byrd Simpson (Berkeley and Los Angeles, 1966); Adriano Prosperi, "America e apocalisse," *Critica Storica*, 13 (1976): 1–61; and Milhou, *Colón y su mentalidad mesiánica*, pt. 2.

⁵⁷ Major, *Christopher Columbus*, 197–98.

⁵⁸ The map is in the Museo Naval, Madrid, Spain. Whether there is more than one Juan de la Cosa has



Figure 4: Detail of Columbus as Christoferens from the map of Juan de la Cosa. Frontispiece reproduced from R. H. Major, ed., *Select Letters of Christopher Columbus* (London, 1870).

cartography have paid much attention to the configurations of the islands and the coastlines, and other technical details, while ignoring the larger religious and cosmological content of the map.

The map represents, in fact, the dissolution of the world picture presented in the medieval *mappaemundi*. The geometrically proportioned disk composed of Europe, Asia, and Africa surrounded by the ocean is gone. In its place is a much more uncertain picture. To the east is a highly articulated drawing of the coast of Western Europe, yet its overall location remains ambiguous. To the west is the emerging coastline of the New World, which the mapmaker can trace with accuracy in only a few places. The interior is unknown and depicted as a vague, swampy, brown-green land mass on which stands a figure—Columbus—carrying the Christ child on his shoulders. The figure is an adaptation of the ancient image of the converted pagan giant, St. Christopher, who carried the Christ child across a swift river on his shoulders—only now it is Christoferens, the bearer of Christ across the ocean to the unknown shores of the New World (see Figure 4).

Ferdinand put this image into words in the biography of his father. Writing about the “mystery” of his father’s name, symbolic of the role he was destined to play in history, Ferdinand explained that “Columbus” meant dove:

If we consider the common surname of his forebears, we may say that he was truly Columbus or Dove, because he carried the grace of the Holy Ghost to that New World which he discovered, showing those people who knew Him not Who was God’s beloved son, as the Holy Ghost did in the figure of a dove when St. John baptized Christ; and because over the waters of the ocean, like the dove of Noah’s ark, he bore the olive branch and oil of baptism, to signify that those people who had been shut up in the ark of darkness and confusion were to enjoy peace and union with the Church.⁵⁹

And Columbus’s given name linked him with St. Christopher:

We may say that just as St. Christopher is reported to have gotten that name because he carried Christ over deep waters with great danger to himself, and just as he conveyed over people whom no other could have carried, so the Admiral Christophorus Colonus, asking Christ’s aid and protection in that perilous pass, crossed over with his company that the Indian nations might become dwellers in the triumphant Church of Heaven.⁶⁰

In a letter dating from his later years, Columbus himself wrote that he had

caused controversy. A Juan de la Cosa was the owner of the Santa Maria and second in command on Columbus’s first voyage. A Juan de la Cosa, a well-known mapmaker, accompanied Columbus on his second voyage and subsequently sailed with Amerigo Vespucci. On the controversy, see Taviani, *Christophe Colomb*, 294–96, 302. On the map itself, see G. E. Nunn, *The Mappemonde of Juan de la Cosa* (Jenkintown, Penn., 1934).

⁵⁹ Ferdinand Columbus, *Life of the Admiral Christopher Columbus*, 4. Also see Las Casas, *Historia de las Indias*, 43.

⁶⁰ *Ibid.*

discovered the “new heaven and the new earth” prophesied in the Apocalypse: “God made me the messenger of the new heaven and the new earth of which he spoke in the Apocalypse of St. John after having spoken of it through the mouth of Isaiah; and he showed me the spot where to find it.”⁶¹ In his mind then, the New World was identified with the end of the world—the first heaven and earth were passed away, there was no more sea—and the journey of the *viator*, which had begun in the deserts of the Old Testament prophets, was surely almost over.

⁶¹ Columbus, as quoted in Spotorno, *Memorials of Columbus*, 224. Later in the same letter Columbus again spoke of the “New World”: “My confidence in God and her Highness, Isabella, enabled me to persevere. . . . I undertook another voyage to the new heaven and earth, which land, until then, remained concealed”; *ibid.* 225.

Reviews of Books

GENERAL

DANIEL J. BOORSTIN. *The Discoverers*. New York: Random House. 1983. Pp. xvi, 745. \$25.00.

The Discoverers is very long. It is split into four books—time, the earth and the seas, nature, and society—which are further divided into fifteen parts and eighty-two sections covering topics from Babylonian hepatoscopy to Keynesian economics. There are discussions of European voyages of exploration that opened up a new world and Chinese voyages that did not, major discoveries that shaped modern science and lines of inquiry that led nowhere, and findings of historians and archaeologists that made sense of the past and insights of other social scientists that do the same for the present.

Some of the best parts of the book trace the discovery of important instruments of discovery—the clock, the compass, the telescope, the microscope, and the printing press. Daniel J. Boorstin talks about what was supposed to be done with these devices, as well as what they were actually used for, and he has a sharp eye for how the same invention was modified by different practices in different places. In the case of printing, for example, he brings out the contrast between Asian and European preoccupations—with, respectively, getting the text right and getting many copies made—by citing Korean regulations that prescribed flogging as punishment for typographical errors. This, he remarks, “helps explain both the reputation for accuracy of the earliest Korean imprints and the difficulty that Koreans found in recruiting printers” (p. 507).

But Boorstin's main concern is with discoverers, not the tools of their trade. His cast of characters is enormous, and there is no shortage of familiar names: Cheng Ho and Columbus; Copernicus and Galileo; Harvey, Darwin, and Pasteur; Feng Tao, Pi Sheng, and Gutenberg; and Clerk Maxwell and Einstein. Boorstin treats all these figures and many others with insight and wit. Specialists may fault him on points of more or less significant detail, but this is decidedly not one of those synthetic works that the

Darwin scholar, for example, will judge poorly informed on evolution yet admirable in its treatment of Mercator and his projection.

In addition to being a large book, *The Discoverers* has a large theme: intellectual progress. As Boorstin explains in a prefatory note, he shows “countless Columbuses” overcoming great obstacles—primarily “illusions of knowledge”—and opening up “the world we now view from the literate West.” Comparative historians will probably dismiss this angle of vision as incurably ethnocentric; they will ask why the world view of the literate West should have unquestioned pride of place. Intellectual historians, with or without comparative bent, may be put off by having the materials they study relegated to the role of impediments to progress; they will question whether there is much to be learned from someone who characterizes his effort to describe once “current ‘facts’ and dogmas of the learned” and “the received common sense” of past times as an attempt to recapture illusions (p. xv).

On both counts Boorstin has the better of the argument. In the fields that interest him, facts can be distinguished from fantasies. The earth is not at the center of the universe, it is not stationary, and it is not flat. In a general history of ideas about time, geography, astronomy, biology, and so on, the “view from the literate West” has to be the center of attention. It is, as they say, no accident that American-trained Chinese scientists outnumber Chinese-trained American ones. Similarly, the fact of Western expansion makes it worth asking why the Arabs did not circumnavigate Africa, if that helps explain why the Europeans did, even though Islamicists may not find the question very useful for their work or, worse, may regard it as an offensive remnant of colonialism, a sign that Americans and Europeans still do not respect the integrity and achievements of other societies and cultures.

The problem with *The Discoverers* lies elsewhere. Intellectual progress was an appealing concept because it promised to do multiple duty and to make sense out of the present and the future as well as the past. The idea still provides a plausible organizing

theme for understanding history, as Boorstin shows. But the reassuring derivation of the way we think now, from progress made by our forebears, no longer seems instructive about our current situation or useful for considering what to do next. If past, present, and future are interesting mainly as milestones along a highway of intellectual achievement, then the record of advance along that road is, as Boorstin would have it, a compelling "story without end," and the fact that "all the world is still an America," that is, "terra incognita," only serves to guarantee that cartographers will never be unemployed (p. xvi). The trouble is that the territory in which we are now lost is not all that uncharted. It is, if anything, too well known for comfort, and the indications are that the pressing need is to change the terrain itself, not continue to rework our maps. Insofar as that is the case, Boorstin's particular tale becomes more repetitive than endlessly informative. The history of intellectual progress has, as the book's dust jacket says, the quality of "a mystery story played by a vast cast on an ever-changing scene." After a while, all but the most addicted readers of detective fiction develop some sympathy with Edmund Wilson's old question: "Who cares who killed Roger Ackroyd?"

PETER BUCK

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CHANDRA MUKERJI. *From Graven Images: Patterns of Modern Materialism*. New York: Columbia University Press. 1983. Pp. xiv, 329. Cloth \$30.00, paper \$12.00.

In this study of the origins of Western materialism, Chandra Mukerji argues that our preoccupation with goods and consumption is evident long before the Industrial Revolution. The production of printed images before 1800 attested and fueled this trend. Mukerji examines mainly the fifteenth through the eighteenth centuries in four case studies: the production of printed works of art, map making, scientific communications, and printed textiles. Her main point is that these goods helped standardize demand, diffuse cultural ideals from the aristocracy down through lower economic levels, and shift trade patterns as instruments of rational calculation.

Although these contentions are plausible and consistent with the evidence, Mukerji's arguments for them are not well constructed. The book as a whole, including the bibliography, creates the impression of a rather loose aggregation of theoretical material from secondary sources with little conceptual cohesion. The chapter on scientific materialism seems especially thin, both in its internal argument and in its relationship to the rest of the volume. The

opening paragraphs of chapter 6, in which the author argues that printed cottons were the first economically significant use of printing, seem to contradict Mukerji's arguments in previous chapters. In subject treatments, the author seems to lack depth, especially in conceptual and technical mastery of the historical cases she examines. Standard scholarly concepts such as "economy of scale" and "democratization of fashion" are noticeably missing from discussions where they would seem appropriate (in chaps. 2, 5, and 6).

Of the four cases in *From Graven Images*, the study of cartography is the strongest. Here Mukerji argues cogently and persuasively that maps were goods "designed to act as economic tools" (p. 81). The printing history and distribution of maps in the Renaissance and Reformation is well treated and could stand alone as a paper on the history of communications.

The weakest case study, by contrast, is that of printed cottons (chaps. 5 and 6). Here, as elsewhere, the flow of discourse seems choppy and disjointed, but the most disturbing aspect of Mukerji's treatment of textiles is its lack of technical depth and competence. Technical errors abound: yarn and thread are persistently confused (pp. 220–22), the comparison between English and Indian textile converting is procedurally mismatched (p. 229), the dye properties of wool are misrepresented (p. 212), and the expression "cotton wool" is employed to refer to lint or raw cotton fiber (p. 238). More important, major issues raised by Mukerji's arguments are not addressed. She fails, for example, to differentiate between opportunities for entrepreneurship open to male versus female textile artisans and omits any discussion of figured textiles other than printed goods and velvets. One is left to wonder how production-woven images such as those in French silks and English and Oriental carpets could be included in Mukerji's theoretical framework. It is not clear whether these questions are omitted out of ignorance or from a disinclination to include counterexamples to the main argument.

From Graven Images includes much that is better handled elsewhere and throws little new light on its subject areas, with the possible exception of the chapter on cartography.

RACHEL MAINES

Clarkson University

ROBERT WILLIAM FOGEL and G. R. ELTON. *Which Road to the Past? Two Views of History*. New Haven: Yale University Press. 1983. Pp. vii, 136. \$14.95.

This brief book consists mainly of two short essays. In some sixty-five small-sized pages, American economic historian Robert William Fogel attempts to

distinguish what he calls "scientific history" from "traditional history." English Tudor historian G. R. Elton offers a rebuttal—or at least questions the distinction—in some fifty pages. A brief introduction and conclusion by the two men round out the short volume.

Fogel tries to lay out an analytical model for the type of history associated with him. According to Fogel, scientific (or social-scientific or cliometric) history differs from traditional history by its preferences for (1) a systematic rather than a stochastic description of its subject matter; (2) the collective and general over the unique and particular; (3) quantitative over literary evidence; (4) explicit analytical models over implicit assumptions and frameworks; (5) controversy over polite revision; and (6) multiauthored, collaborative projects over a solitary, individualistic work style. He does allow that behavioral models ought to be place and time specific, but, in the end, Fogel hypothesizes two paradigms or modes of historical research in conflict and their practitioners in "cultural warfare" (p. 65). In spite of the differences he postulates, Fogel argues that the two forms of doing history are complementary.

Elton agrees that the two kinds of history, if they can be viewed that way, show more common ground than fundamental differences. In fact, he questions whether the criteria Fogel uses to distinguish the two forms of historical practice are the really essential ones in doing history. Neither the types of evidence nor the generality or uniqueness of the subject distinguish good from bad historical practice. Moreover, traditional historians seldom use the legal model of evidential validity, no matter what the handbooks say. In the end, Elton believes that historical truth can be attained by reference to past facts. He believes that Fogel is too hopeful about the achievements of so-called scientific history.

The two distinguished historians favor good over bad history and facts over interpretations in their joint conclusion. That the two authors so readily agree to disagree and "respect each other" leads the reader to conclude that they share a larger framework of presuppositions about the nature of history. They espouse, if I may coin a phrase following Kuhn, "normal history" and operate within the same paradigm of assumptions guiding the overwhelming number of British and American historians. Thus, to play with their title, the reader finds not two separate roads in this slim volume but a single road with, at most, a painted line down its center. The road leads to the very same mountain of empirical fact for both Elton and Fogel, because, as positivists on the larger issues, they subscribe to what modern philosophers of history call "naive realism" in the historical enterprise. In asserting the primacy of fact over interpretation, they espouse the "historical fundamentalism" dominant in the American

and British historical profession in this century. Other historians see not only other roads today but also roads that lead to quite different places than the destination of Fogel and Elton.

As an intellectual artifact, therefore, this little book is a peculiar anomaly in our time. On the one hand, the two authors argue as if the history profession in the United States (and in England by inference) were still in the 1960s, when quantitative history was an exciting new development, promising a new era of historiography. On the other hand, the book seems to lack a third author who would point out the several roads (and mountains) that have been found in the American and British, not to mention the European, intellectual worlds during the past two decades. If the two authors neglect the implications of Fernand Braudel and Emmanuel Le Roy Ladurie for doing history, how can they see the challenges of Michel Foucault and Hayden White?

ROBERT F. BERKHOFFER, JR.
University of Florida

DEAN KEITH SIMONTON. *Genius, Creativity, and Leadership: Historiometric Inquiries*. Cambridge: Harvard University Press. 1984. Pp. ix, 231. \$20.00.

This book is a historiometric discussion of creativity and leadership. Historiometry is defined by Dean Keith Simonton as "the method of testing nomothetic hypotheses concerning human behavior by applying quantitative analyses to data abstracted from historical populations" (p. 3). The author, striving for "scientific" explanations, emphasizes "universals of human behavior," that is, behavior that is not contingent "on the geographical location, transhistorical period, or personal idiosyncrasies of an individual creator or leader" (pp. 3–4). According to the author, the success of such historiometric work, "isolating nomothetic principles," depends on sophisticated procedures of data analysis as well as on techniques of quantification (p. 12).

The most important lesson anyone can learn from this study is the truthfulness of the old saying, "If it can be measured it isn't important; if it's important, it can't be measured." Let us grant that the data base the author used allows him to establish statistically significant relationships between, say, birth order and artistic activity or birth order and revolutionary activity (p. 27). We might well ask why any of this matters or what difference it makes if artists are more likely to be first-born children whereas revolutionaries are more likely to be middle children and less likely to be only children. The observed relationships do not by themselves reveal anything significant, and they acquire significance here only by virtue of inferential leaps based on the author's use of certain regulative constructs of psy-

choanalysis, social psychology, and cognitive psychology, as well as everyday common-sense thinking. Thus, we find statements such as these: "If the father dies or is often absent from home, the son may fail to identify fully as a male and instead become more intimate with the mother. The outcome could be an androgenous and even slightly feminine male, a type of personality that may be conducive to creativity." Or, "Perhaps . . . despots and conquerors have the same basic personality as poets but cover it with a veneer of machismo." Or again, "The way eminence transfers from generation to generation is best explained as an example of role-modeling. Each king can imitate or identify with his male predecessors in the royal line" (pp. 29, 32). The book is replete with such statements, which constitute attempts to make sense of quantified results by reference to human subjectivity but which then have little to do with nomothetic principles or science.

The point is that historiometric—or even less grandiose quantitative—studies can succeed at all only if they deal with simple problems (the kind that allow the author to conclude "that certain prolific persons are responsible for a disproportionate share of the achievements in any given endeavor and that this quantity of productive output is probabilistically connected to the quality of impact, or eminence") (p. 181). Attempts to deal with more complicated problems founder on the observed contradiction, the need to understand human subjectivity in the absence of any generally accepted conception of what is involved, let alone how to measure it. The author, concentrating on quantitative analysis, never demonstrates that human subjectivity, particularly in the kinds of complicated situations historians deal with, is amenable to such analysis. Hence the fundamental critique of this study and studies like it: they are suspect on epistemological grounds, and they focus attention on the wrong issues.

FRED WEINSTEIN
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NORMAN LEVINE. *Dialogue within the Dialectic*. Boston: George Allen and Unwin. 1984. Pp. 416. \$37.50.

Norman Levine argues that from Marx to Mao the dialectic, as derived from Hegel, forms the core of Marxist thought, because it is both a method of social analysis and a guide to human action, the two constituting a dialectic of action. Marx's dialectic of action is historical materialism. In the hands of Engels, Plekhanov, and Stalin, however, the dialectic became dialectical materialism, a dialectic of nature. Nevertheless, Lenin and Mao, in the course of their respective careers as Marxist revolutionaries, came

to understand the dialectic as a dialectic of action rather than as a dialectic of nature. This dialectic of action, continues Levine, is the proper Marxian dialectic. Its renewal as both a method of social analysis and a guide to human action is the basis for "a new theoretical appropriation of the contemporary world" (p. 404).

Traditional academic historians may look askance at Levine's argument, but he carefully grounds it in textual analyses and provides correct historical contexts. In separate chapters Levine shows that Marx confirmed his historical materialism through his reading of Hegel's *Science of Logic* and *Encyclopaedia of the Philosophical Sciences*; that the same *Science of Logic* and Hegel's *Philosophy of Nature* confirmed for Engels his dialectical materialism; that a close textual comparison between Marx's original version and Engels's edited version of section 1, volume 2 of *Capital* reveals the difference between historical materialism and dialectical materialism; that Lenin, in his *Philosophical Notebooks*, after 1914 moved away from the dialectic of nature to the dialectic of action; and that Mao, by the late 1930s, also came to a correct understanding of the dialectic of action.

Levine ably uses textual analysis and historical context to demonstrate the cogency of his argument. One may quibble over minor points of interpretation, but on the whole Levine's scholarship is sound. *Dialectic within the Dialectic* is not the usual academic "history," because it does not arrive at a conclusion but starts out with an argument. Yet Levine, by following the ground rules of historical method, convincingly argues his thesis. As radical-political historiography, the book is a success and a challenge to traditional academic historiography.

DONALD M. LOWE
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MARGARET JACOB and JAMES JACOB, editors. *The Origins of Anglo-American Radicalism*. Boston: George Allen and Unwin, for the Institute for Research in History, New York. 1984. Pp. x, 333. \$35.00.

This volume of mostly original essays is concerned with the general questions of "what was radical, what did it mean to be radical, . . . what kinds of radicalism were there in England and America before 1800," and what "connections . . . can be found between radicalism in England and America" (p. 1). This broad definition of the problem has permitted individual authors to range over a wide spectrum of developments that questioned or challenged anything that might have been considered "essential or fundamental" (p. 107) to the societies under consideration.

Of nineteen chapters in the volume, seventeen are substantive essays and two, by David Und-

erdown and James A. Henretta, are brief commentaries. Nine of the substantive essays deal with England, six with America, and two with subjects that might best be described as transatlantic. Six of the essays on English developments are concerned with the seventeenth century. T. Wilson Hayes surveys the radical aspects of the Familists; Phyllis Mack explores the emergence of women as prophets during the English Civil War; Mark A. Kishlansky presents a persuasive case for the centrality of the goal of consensus in the Putney Debates; Christopher Hill considers the possibility that following the Restoration Civil War radicals may have emigrated to the colonies or joined the egalitarian ranks of one of the many groups of pirates. In two related essays, Corinne C. Weston contends that the triumph of the principle of coordination—the idea that sovereignty lies not just in the Crown but in the Crown-in-Parliament—was the most radical political achievement of seventeenth-century England, and Lois G. Schworer argues that at least in part because it embodied that principle the Declaration of Rights was far more radical than traditionally depicted.

Essays by J. G. A. Pocock, Nicholas Rogers, and Robert W. Malcolmson challenge the conventional conception of eighteenth-century England as “an intrinsically consensual, complacent and deferential society under aristocratic leadership” (pp. 132–33). Focusing on the nature of the ideological differences between “ruling Whiggism” and “radical Whiggism” (p. 35), Pocock illustrates the extent to which “the language of the latter was often reiterated by Georgian oppositional Toryism” (p. 49). In a complementary study, Rogers analyzes the strength and nature of urban opposition to the Whig regime between 1720 and 1760, while Malcolmson discusses the development and character of workers’ combinations.

All six of the essays on America deal primarily with eighteenth-century developments. In separate essays, Patricia U. Bonomi and David S. Lovejoy discuss the radical impact of religious enthusiasm on the American sociopolitical order, and Rhys Isaac, building on his extensive work on religious dissenting culture in Revolutionary Virginia, proposes an interpretative framework for analyzing the character of such collective movements. Two essays, one by Gary B. Nash on urban artisans and the other by Steven Rosswurm on the process of politicization and mobilization during the War for Independence, illustrate both the character and strength of radical impulses in Revolutionary Philadelphia. In one of the most imaginative and original pieces, Alfred F. Young uses four tableaux to illustrate the ways in which English plebian culture was transmitted, retained, and transformed in eighteenth-century America.

Finally, essays by Joyce Appleby and Richard J. Twomey consider aspects of the relationship between radical developments in England and America. Appleby examines the intellectual origins and implications of what she identifies as “the most radical concept in eighteenth-century Anglo-American thought,” specifically, the notion that national self-government could be “based upon the self-governing capacities of the individual citizens who composed the nation.” Twomey assesses the role of English and Irish radicals in American political life between 1790 and 1810 and recounts the impact of their American experiences on their thought and lives.

Although the failure to include any consideration of the antislavery movement, surely the most radical challenge to the existing Anglo-American order during the era covered by the volume, constitutes a serious omission, these essays, as the editors and Appleby suggest in an excellent brief introduction, significantly enhance our understanding of “the range of human concerns that could provoke . . . people to contend with those in power” (p. 10). Perhaps even more important, they illustrate how limited radical aspirations were in two of the more prosperous, more expansive, and least repressive segments of the preindustrial world, and they cast considerable light on the reasons why those aspirations did not go further. If, instead of following the format of the 1980 conference out of which it developed, this volume had been organized according to the sequence of the discussion in the introduction, it would probably have functioned more effectively as a book. But the high quality of the essays will amply repay the close attention of all students of early modern Anglo-American history.

JACK P. GREENE
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ERNEST GELLNER. *Nations and Nationalism*. (New Perspectives on the Past.) Ithaca: Cornell University Press. 1983. Pp. viii, 150. Cloth \$24.50, paper \$6.95.

Periodically, an important book emerges that makes us, through the uniqueness of its theory, perceive history as we have not seen it before. Ernest Gellner has written such a volume. Students of nationalism will have to come to grips with his interpretation of the causes for the emergence of nationalism, since he has declared that most of the previous explanations are largely mythical.

Gellner is a prolific writer and is considered an important intellectual figure in interpreting contemporary society. Although a philosopher by profession, he has also written about history, sociology, and anthropology. This book is the first in a series

titled "New Perspectives on the Past," under the general editorship of R. I. Moore. Gellner asserts that conventional wisdom holds that there are peoples who are inevitably members of a common society or nation. He insists that this is largely a false belief. The reality is that the industrializing state creates nationalism in order to achieve the homogeneous society the state needs to prosper. "Nations as a natural, God-given way of classifying men, as an inherent though long-delayed political destiny, are a myth; nationalism, which sometimes takes pre-existing cultures and turns them into nations, sometimes invents them, and often obliterates pre-existing cultures: *that is a reality*" (pp. 48–49).

To understand why nationalism emerged during the last two hundred years, Gellner begins by examining preindustrial, agrarian society. It was noted for its small elite (the power holders) and the illiterate mass of those who were ruled. As this agrarian society evolved into an industrialized one, the need increased for a citizenry that was literate, mobile, spoke a common language, and held the same cultural values. Industrialized society is complex and requires a generically educated population that can move about to perform functions necessary for the state to succeed. In this setting, nationalism grows as the population of a state becomes homogeneous in thought and mores.

Central to the growth of nationalism, then, is the state's educational system. The educator imparts the values common to all citizens. Thus, the most important attribute of the modern nation-state may not be its monopoly of violence but its monopoly of legitimate education. Gellner implies that the rise of nationalism cannot be explained by cultural developments, such as modest literary revivals, à la George Antonius. This is romantic nonsense. "Mankind is irreversibly committed to industrial society" (p. 39), and one of its products is nationalism.

One might respond by saying that nationalism exists and has existed in numerous nonindustrializing states. This book's importance may lie in its description of what happened in Western Europe and its offshoots. It is of less value in interpreting, for example, the Chinese experience. Although Gellner's monograph is not the final answer to the question of the rise in nationalism, it certainly tells us that the issue is not closed. I look forward to reading the rebuttals.

WILLIAM W. HADDAD
Illinois State University

JOHN WALTON. *Reluctant Rebels: Comparative Studies of Revolution and Underdevelopment*. New York: Columbia University Press. 1984. Pp. xii, 230. Cloth \$27.50, paper \$10.00.

JAROSLAV KREJČÍ. *Great Revolutions Compared: The Search for a Theory*. Assisted by ANNA KREJČOVÁ. New York: St. Martin's. 1983. Pp. xii, 251. \$27.95.

A decade ago, I read a book that promised to compare revolutions from Cromwell to Castro. Disappointingly, the only possibility for comparison lay with the reader, who had to undertake the work the author chose to ignore. Authors who dare to include the word "comparative" in their titles owe it to readers to write with the express purpose of noting similarities and differences. Further, comparative studies of revolution demand that authors possess kaleidoscopic interests and a panoramic view of history. These two books actively compare, and John Walton and Jaroslav Krejčí are appropriately equipped. Ultimately, however, one succeeds, but the other does not.

Walton and Krejčí are sociologists whose approaches are historical. Walton is concerned with rebellion and underdevelopment, and the three rebellions he studies as prototypes (Huk, La Violencia, and Mau Mau) convincingly suggest that their causes and consequences closely resemble successful revolutions. Krejčí seeks to develop a morphology of revolution in which he assesses regularities in revolutionary processes by referring to six "great" revolutions. His main contention is that divisions among revolutionists, their shifting alliances, and their confrontations determine the course of revolutions. But the value of the model he constructs is unclear, especially since it does not enhance our understanding of revolutions.

Walton's book is comparative history at its finest. Generalizations are supported by references to all three case studies, and these are compared directly both to one another and to other revolutions, major and minor. With clear, readable prose, Walton makes a strong case for his claim that, qualitatively, national revolts—which are more common and sufficiently similar in their origins and causes—are of the same genre and belong to the same theoretical field as great revolutions. Such factors as alienation of land and national organization of popular resistance are as germane to his national revolts as to the revolutions in France, Russia, China, and Cuba. Walton makes ample and documented use of a wide variety of recent research to house and decorate his hypothesis.

Contrarily, little that is instructive emerges from Krejčí's comparative references. His book is not much more than a cursory and familiar review of six revolutions, all fitted into a prefabricated mold. The positions of other scholars are restated as he examines the stages of revolution, but this has been done more effectively elsewhere. There is no new research, key publications are largely ignored, and precious few insights are offered. Krejčí's conclu-

sions are more obvious than provocative (for example, "Once a revolution breaks out, then it is likely that it will take a considerable time [several decades] before issues other than those which caused the revolution assume greater importance" [p. 213]). Even the writing style is a hindrance. Often what should be clear and straightforward becomes, owing to Krejčí's proclivity for excessive jargon (for example, "civilisational matrix of revolution"), complex, convoluted, and obfuscated.

Walton's contributions are many and universally applicable. He characterizes the kinds of revolutionary situations he has presented by examining, in particular, how they arise out of general conditions of underdevelopment. He sees, for example, both the pauperization of urban workers and the exploitation of the peasantry as stemming from the expansion and penetration of the global, capitalist, political economy—the expansion of First-World politics and economics to the Third World. By analyzing postrevolutionary political struggles, he also explains why some anticolonial revolts choose a socialist path. And the parallels he draws have implications for current and potential conflicts in Central America and the Middle East.

Instead of deriving uniformities of revolution (as did Crane Brinton), Walton seeks to discover similarities and degrees of difference. Instead of quibbling about what is or is not a revolution, he confronts the issue by demonstrating how such debates are not only invalid but also unnecessary. The only missing ingredient is ideology; these national revolts are significantly different from great revolutions in terms of their ideals and their goal to transform the myth of the social order. But this gap does not detract from his otherwise excellent book.

Krejčí addresses ideological issues satisfactorily, but this is not enough to redeem a study that goes nowhere in particular—although it does so with determination and conviction. All he seems to do is label the various phases of development and the shifts in the centers of power of the revolutions he studies. This then becomes his morphological model.

The model is tested with reference to six great revolutions. Although most would agree that the English, French, Russian, and Chinese revolutions were "great" indeed, historians may question the wisdom of including the Czech Hussite and Turkish national revolutions. Also questionable is Krejčí's application of dates to encompass his revolutions: English (1628–89), French (1751–1884), Russian (1818–1982), Chinese (1850–1982). He is obviously attempting to set these countries and their many and diverse revolutions into larger, more meaningful periods in order to include long-range causes and the resolutions of issues raised by the actual revolutions. But, instead of helping, his methods

and model metamorphose into what I can only think of calling *bouillabaisse* history; a lot of it looks familiar but has no clear characteristic of its own. This is not an effective way to acquire a taste for comprehending revolutions.

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DONALD DENOON. *Settler Capitalism: The Dynamics of Dependent Development in the Southern Hemisphere*. New York: Clarendon Press of Oxford University Press. 1983. Pp. iv, 280. \$49.50.

Ideally, economic history should serve two masters: theory and history. In practice, most of us serve one and neglect the other. At the moment theory is king. But not so for Donald Denoon, who, in his study of the dynamics of development of white-settled communities in the Southern Hemisphere, is at pains to stress the historical angle—economic and otherwise. The result is a book that would have pleased Adam Smith.

Like Smith, Denoon is wide-ranging. Convinced that the dynamics of the countries he is studying—Argentina, Chile, Uruguay, South Africa, Australia, and New Zealand—can only be revealed by a comparative analysis, he begins by examining the standard theoretical approaches to economic development. Attempting to relate theory to historical fact, he then surveys the demographic, economic, social, and political changes taking place during the nineteenth and twentieth centuries. He concludes his search for common features in the historical transformation of these six societies by deciding on "a settler capitalist mode of production" as the decisive factor explaining why his white-settled communities prospered as they did.

The value of the book is that it provides us with a comparative history (and the same analytical framework) for countries about which many of us know very little. Its essential contribution, however, lies not in the newness of its material or of its ideas but in its method—its refusal to be limited to any one discipline or region.

This unusual approach is not without rewards. By illustrating the diverse conditions under which development took place in the six countries—even where a marked degree of social unity and external stimuli existed—it demonstrates once more the complexity and sometimes the paradoxical nature of economic development. Furthermore, its scrutiny of existing theories against the background of historical change over such a wide area is timely. Especially important is the idea that development takes place in a fixed order, rigorous and compelling, or that economic development or economic

backwardness is the result of European expansion and world ascendancy. Not the least of its benefits is the stress the author places on society as a whole.

Of course, there will be those who will sniff at the lack of primary sources. Others will demure at the breadth of the subject. Still others will wonder if Denoon does not sometimes set up an "Aunt Sally" in order to have the pleasure of knocking her down again. The basic question is whether the author's notion of "a settler capitalist mode of production" is viable. We erred in the past by overstressing trade as the *primum mobile* in economic development; more recently we erred by overstressing technology. Must we repeat the error? Limitless and complex subjects such as those examined here rarely do have a simple, monistic explanation. Still, Denoon's courageous attempt to illuminate economic development in the Southern Hemisphere has undoubtedly left us in his debt.

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ANCIENT

W. D. DAVIES and LOUIS FINKELSTEIN, editors. *The Cambridge History of Judaism*. Volume 1, *Introduction; The Persian Period*. New York: Cambridge University Press. 1984. Pp. xv, 461. \$59.50.

The editors, W. D. Davies and Louis Finkelstein, open their history with what they intend to be their controlling definition of Judaism: "the form which the religion of Israel assumed in and after the Babylonian exile." Then, however, they immediately distinguish between Jewish and Gentile scholars, thereby asserting that Jews are a people in contrast to other peoples (Gentiles) and that the history of Judaism, therefore, is really a history of the Jewish people. The book indeed wavers between being a history of the Jewish people (however that may be defined) and a history of Judaism, and the editors seem to believe a distinction between those two subjects is neither necessary nor possible.

Moreover, this volume in the four-volume *Cambridge History of Judaism* can scarcely be called a history. Rather, it contains sixteen studies that are preliminary to a history of Judaism in the Persian period (539–330 B.C.E.). The editors, acknowledging "the intrinsic difficulty of [their] whole enterprise," tell us that they have intended "to bring together the two seas of Jewish and Gentile scholarship" and take account of "new sources of knowledge gained in this century." They gratuitously conclude that "a 'definitive' *History of Judaism* . . . is an impossibility." The volume has been a decade in preparation. Two chapters were written in 1973, the

preface is dated February 1982, and the volume appeared in 1984.

The book begins with a superb chapter by Denis Baly, "The Geography of Palestine and the Levant in relation to Its History." It provides an indispensable description for anyone interested in Near Eastern history. Equally helpful is Peter Ackroyd's essay, "The Jewish Community in Palestine in the Persian Period," which summarizes and emphasizes the relatively meager evidence and the "great problems of [its] interpretation." The chapter effectively presents what can be said with certainty about Jews and Judaism in the Persian period. The longest chapter (sixty pages), and in many ways the most stimulating, is "Jewish Religious Life in the Persian Period" by Morton Smith. He also emphasizes "the extent and variety of our ignorance" but nevertheless repeats novel theories he had earlier propounded in his *Palestinian Parties and Politics That Shaped the Old Testament* (1971).

Summary studies—"Persian Religion in the Achaemenid Age" by Mary Boyce, "Babylonia in the Persian Age" by M. Dandameyev, and "Egypt, Persian Satrapy" by Edda Bresciani—provide necessary background for an understanding of the Persian era itself in the Near East. "Iranian Influence on Judaism: First Century B.C.E. to Second Century C.E." by Shaul Shaked and "The Jews in Egypt" by Bezabel Porten are two important, if at times highly speculative, studies that suggest some of the dimensions of the historical problem. Useful information and speculation on numismatics (Uriel Rappaport), calendars and chronology (Elias Bickerman), the archaeology of Persian Palestine and its political and social history (Ephraim Stern), Hebrew and Aramaic in the Persian period (Joseph Naveh and Jonas Greenfield), prophecy and psalms (Gunter Wanke) and "Wisdom literature" (Hartmut Gese) in the Persian period, and a chapter on the Babylonian captivity (Elias Bickerman) that should contribute to a more comprehensive survey—all reveal the difficulty of defining the historical problem adequately and the consequent impossibility of describing the beginnings of Judaism. The book might well be entitled "Preliminary Essays on the Origins of Judaism and the Jewish People." Materials and hypotheses that cogently direct our attention to the historical problems and let us trace the growth of Judaism in the Persian period do not yet exist.

Full bibliographies, an index, chronological tables, maps of Palestine and the Levant, and plates for the chapter on numismatics round out the beautifully produced volume. Some printing errors escaped the authors and editors, for example, "millennium" (pp. 336, n. 1., 391), and "Indication of date are given" (p. 258).

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WOLFGANG WILL. *Athen und Alexander: Untersuchungen zur Geschichte der Stadt von 338 bis 322 v. Chr.* (Münchener Beiträge zur Papyrusforschung und Antiken Rechtsgeschichte, number 77.) Munich: C. H. Beck. 1983. Pp. viii, 175. DM 73.

Even in periods of intense scholarly interest in the problems of a particular historical epoch, important subjects are often ignored. As Wolfgang Will rightly points out in the introduction to his monograph, such has been the fate of Athens during the reign of Alexander the Great. Will's study of the history of Athens from the battle of Chaironeia in 338 B.C. to that of Amorgos in 322, which marked the end of Athenian naval power, is, therefore, welcome, especially since Will offers a bold and original new interpretation of Athenian history during these years.

According to Will, in the years immediately following the Macedonian victory over Athens and Thebes at Chaironeia in 338, Philip II and then Alexander actively and successfully courted Athenian support. As a result, pro-Macedonian politicians gradually gained the upper hand at Athens, and, under Lycurgus, Athens followed a policy of close cooperation with Macedon, a policy that brought the city renewed prosperity and preeminence in the Greek world until Alexander's exiles decree in 324 rendered such close and mutually advantageous relations with Macedon impossible. Will is thoroughly familiar with all the relevant ancient and modern scholarly literature, and he argues his case clearly and forcefully. He is, moreover, certainly correct to emphasize in support of his thesis the courting of Athens implicit in such actions as Alexander's dedication of the spoils from Granicus there instead of at some Panhellenic center such as Olympia and the essentially nonmilitary character of much of Lycurgus's great program of reform.

A short review does not allow the full discussion that so striking a thesis requires. Nevertheless, despite the clarity and vigor with which Will argues his case, three areas of difficulty should be noted. First, his neat division of Athenian politicians into pro-Macedonians and fanatical anti-Macedonians is too simple and is belied by his own narrative, which suggests that the principal political leaders were primarily pragmatic Athenian patriots whose actions were guided by realistic assessments of Athenian interests and the possibilities of realizing them at any given time. Second, at times he yields too easily to the temptation to argue away evidence potentially damaging to his thesis, such as his facile dismissal as a late fourth-century invention of the claim in *SIG* 3326 (compare pp. 98–99) that Lycurgus was an opponent of Macedon. Third, and finally, he repeatedly refers to the hope for "economic" benefits as one of the goals of Athens's

support of Macedon in this period, but he offers no adequate analysis of this alleged aspect of Athenian policy. Despite these flaws, *Athen und Alexander* is significant monograph that will be of interest to all historians.

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N. G. L. HAMMOND. *Three Historians of Alexander the Great: The So-Called Vulgate Authors, Diodorus, Justin, and Curtius.* (Cambridge Classical Studies.) New York: Cambridge University Press. 1983. Pp. xi, 205. \$39.50.

There is no dissent from N. G. L. Hammond's claim that the main problem of scholarship on Alexander is to assess the value of the sources. There is, however, little agreement on anything else, as scholars have struggled to identify the sources that underlie our major extant accounts of the Macedonian king.

Most scholars separate the sources into two groups. One originates in the memoirs of those who—like Ptolemy and Aristobulus—had accompanied the king in the field. The second group—known as the "Vulgate" tradition—represents popular, often sensational, but not eyewitness, literature. The best known Vulgate writer was Cleitarchus, whose account of Alexander was the most popular in the ancient world.

In his recent book, *Alexander the Great: King, Commander and Statesman*, Hammond relied heavily on the account of Arrian, a second-century commander and provincial official, as the best surviving narrative on Alexander. Much of Arrian is based on Ptolemy. It is astonishing that Hammond accepts totally Arrian's view that Ptolemy is a reliable source, not only because he had accompanied Alexander, but also because he himself was a king for whom it would be dishonorable to lie. Kings, especially a King Ptolemy much of whose legitimacy rested on his connections with Alexander, might have had all the more reason to lie. Ptolemy's honesty should be as suspect as that of any other author.

Thus, Hammond sets up Arrian as a standard by which the other ancient accounts of Alexander should be measured. Three other authors are the subject of this book: Diodorus, a late-first-century-B.C. writer whose section on Alexander forms part of a universal history; Curtius, a mid-first-century author who wrote an independent history of Alexander; and Justin (unknown date), who produced an epitome of a wide-ranging historical narrative by the first-century writer Pompeius Trogus.

Hammond's method is to analyze each account and determine which earlier source was used. If a

passage seems foolish, sensational, or suspicious, the source is likely Cleitarchus. If a passage seems believable either on general grounds or because it accords with what we know about Ptolemy and other "good" sources, it belongs to a non-Cleitarchan tradition.

This is not much of a methodological improvement over what W. W. Tarn did thirty-five years ago. But Hammond has been innovative by showing that our extant ancient writers used sources with a purpose. Hammond's analysis of Diodorus—perhaps the major contribution of the book—is a case in point: Diodorus's account of Alexander is meant to be a centerpiece. Diodorus used Cleitarchus to bring life to the narrative and to make Alexander's achievement seem heroic without flattering him: Roman popular opinion had not yet come around to idolizing Macedonian kings.

For sober institutional matters and for details of Greek and Macedonian affairs, Diodorus used the staid Diyllus, an early Hellenistic Athenian who wrote a general history based on a number of eyewitness accounts. Thus, there are two main traditions in Diodorus, a "good" one from Diyllus and a "bad" one from Cleitarchus.

Critics will have a field day with this book, not only because Hammond's solutions to some problems seem naive, but also because no attempt to unravel the tangle of source problems will satisfy all the devotees of *Quellenkritik*. This reviewer has serious reservations about Hammond's method. But there is also much of value here, including a sophisticated analysis of the sources on Philip II's death and the early years of Alexander's reign, the exposition of Diyllus as a major source for Diodorus, and a shrewd revelation of some military anachronisms in the accounts of Diodorus and Curtius. And the passage-by-passage analysis of the sources for the three authors will require those who think about the subject in the future to consult this book whether in the end they agree with it or not.

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R. E. ALLEN. *The Attalid Kingdom: A Constitutional History*. New York: Clarendon Press of Oxford University Press. 1983. Pp. ix, 251. \$42.00.

With the proliferation of Ph.D.s, the assignment of thesis topics in classical history has increasingly become a gamble: the evidence is likely to be insufficient to treat any not recently treated. Oxford, long known as the home of lost causes, now publishes dissertations on hopeless topics that apparently are judged to have other merits to compensate.

A "constitutional history" (even if we stretch the term to include administrative) of the Attalid king-

dom can never have appeared feasible. The evidence is epigraphic, and there are few inscriptions, mostly fragmentary. R. E. Allen is a well-trained epigraphist (though, surprisingly, he does not know Robert K. Sherk's standard work, *Roman Documents from the Greek East*, and cites those texts from outdated editions). He carefully sorts the thin material and supplements it by hypotheses, some of them new.

His interpretation of the policies of Philetaerus (the founder) and of his successor Eumenes I depends largely on his assertion that the treaty in which Eumenes made concessions to his disaffected mercenaries stationed in two fortresses (north and east of Pergamum) was concluded early in the reign. This again depends on the view that a reference to "the forty-fourth year" in that treaty is to the Seleucid era (hence must precede the break with the Seleucids), not (as may seem more plausible) to a mercenary's age or length of service. This will suffice to indicate the quality of much of the evidence and some of the argument. The early dating, incidentally, prevents Allen from acknowledging that the reference to Eumenes's ships indicates possession of a navy, for on his hypothesis Eumenes cannot have controlled a seaport at this time.

In a revisionist interpretation of the next ruler, Attalus I, Allen makes him out to be essentially unambitious. For a man who, straight after his accession (as Polybius carefully and explicitly attests, although Allen lamely opines that this "cannot be pressed"), challenged the dreaded Gauls and made his victory over them into the legitimization myth of his kingship and his pretensions, this is less than plausible. In fact, Attalus always exploited Seleucid weakness to seize whatever he could, and it is capacious to assert (we have no evidence) that he "failed to establish a political hegemony" over his conquests. He could never retain them for long against a determined challenge, for he had no sound political or demographic base for his rule. At one time—so Polybius tells us, with some exaggeration (quoted p. 38)—he controlled everything this side of Taurus; a little later (p. 36), we find him besieged in Pergamum, with no other possessions left. Yet even before the Seleucids granted him security of recognition in a much reduced kingdom (216 B.C.), we find him buying the friendship of the Aetolian League, which ultimately led to his association with Rome and conquest of Aegina. If, by the end of his life, he had lost most of his conquests, this perhaps shows, not lack of ambition, but lack of ability to fulfill it.

As to Attalus's control of the cities under his rule, we know little in detail, except about Pergamum. But he uses blunter language than any other Hellenistic king about his right to give them orders and must have kept them on a tight rein. We know that

he got what money he could out of at least some of them, in order to use it to make friends and forge connections far from home, with governments and sanctuaries. A remarkable inscription shows how glad Teos was to be relieved by Antiochus III of the tribute it had paid to Attalus (p. 50 and following), and exploitation elsewhere should not be questioned (as, for example, pp. 46–47) by reliance on the courtesies of diplomatic terminology.

With the reign of Eumenes II our literary sources improve, and at least political and diplomatic history occasionally becomes possible. Allen, following Polybius, recognizes Eumenes's skill and exaggerates it; for Eumenes, by the end of his life, had lost Rome's favor to an extent endangering his kingdom. On the other hand, Allen repeats the conventional view (essentially based on a few words in a single inscription) of Attalus II as a weak king, even though he made and unmade kings in Asia Minor and even Syria at his pleasure and raised Pergamum to uncontested dominance, while keeping Roman suspicion disarmed by skillful diplomacy and unquestioning support. It was only his unbalanced successor, Attalus III, about whose short reign we know practically nothing, who ended it all by unaccountably willing his kingdom to an evidently surprised Senate and People of Rome. On this, Allen can throw no more light than many predecessors.

He is at his best in his careful discussion, in the light of the literary and documentary sources, of the settlement of Apamea, which, after Rome's war with Antiochus III, made Eumenes II's kingdom into a major power for the first time, by Rome's grace. He tends to make this the beginning of most administrative institutions—which is plausible, but not demonstrable. It seems (but we do not really know) that governors in Greek cities now become more numerous, in addition to the king's traditional intervention in city elections and finances. Even this is hard to document, except at Pergamum itself (chap. 7). The precise meaning of the term *topoi* ("places") in various documents from Pergamum and in the titles of administrative officers cannot at present be recovered with any confidence. The best chapter, perhaps, is the one on royal cults, which for the first time integrates much recent work and, *inter alia*, documents the irrelevance of divine honors to political suzerainty.

There are fairly numerous typographical errors and occasional mistakes in translations of documents.

The work ends with four appendixes (one of which is a useful selection of inscriptions relevant to the dynasty found outside Pergamum), a bibliography, and a useful, though incomplete, index. The author's chief merit lies in the presentation and

detailed discussion of a large number of documents. The book is intended solely for specialists.

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JÜRGEN MALITZ. *Die Historien des Poseidonios*. (Zetemata, Monographien zur Klassischen Altertumswissenschaft, number 79.) Munich: C. H. Beck. 1983. Pp. 486. DM 122.

Philosopher, philologist, scientist, and historian, Posidonius of Apamea was one of the most eminent and influential Greeks of his time, and for some time it seemed to modern scholars that no Roman of his age had a thought that did not derive from him. Panposidonianism has faded, but there is still little consensus about what may fairly be attributed to him. His voluminous *Histories*, which dealt in fifty-two books with the late second and early first centuries B.C., are represented by only twenty-eight citations explicitly credited to that work by their source, Athenaeus; there are some thirty-three more bearing his name that are clearly historical. Beyond that core there is little agreement. Diodorus Siculus clearly owes much to him, but how much, and how faithful his borrowings, is not easily determined—Diodorus never names Posidonius. Felix Jacoby assigned some long passages of both Diodorus and Strabo to Posidonius, and the most recent editor, W. Theiler, ascribes much of books 33–37 of Diodorus, together with significant sections of Strabo, Plutarch, and others, to the *Histories*. In this lengthy *Habilitationsschrift*, Jürgen Malitz goes considerably beyond even Theiler in finding new pieces of Posidonius; his subjective judgments, given without serious argument, are not likely to meet with uncritical acceptance.

The bulk of Malitz's book (following chapters on Posidonius's life, the problems of reconstruction, and the organization of the *Histories*) consists of exegesis of what he considers genuine remains of the *Histories* and is set out geographically and topically rather than chronologically. Vast quantities of Diodorus and Strabo are presented in German translation, interwoven with annotation and attempts to find the underlying themes of the *Histories*. Malitz's goal is to give a sense of the work as a whole; in particular, he wants to see it in the context of Posidonius's philosophy. He suggests not only that Posidonius wished his work to be ethical and instructive (in common with most other ancient historians) but also that he employed a geographic and climatic explanation of human character, that ethnographic descriptions were prominent, and that he emphasized a Stoic view of divine providence and cosmic sympathy:

Such a view of the *Histories* is certainly possible; the difficulty is that, in these and other matters, Malitz simply has no evidence to support his conclusions, and what he first presents as possible all too rapidly is set forth as fact. In the first chapter Malitz is rightly uncertain about Posidonius's command of Latin (p. 20); later on, he is able to consult Latin sources (p. 361). Following Jacoby, Malitz suggests on the basis of a single fragment that the first two books of the *Histories* may have been an ethnographic introduction to Italy and Rome (p. 64); near the end of the book we learn on the basis of no new evidence that books 1 and 2 explained Roman superiority in terms of Rome's military strength and of the climatic factors that made Rome's rise possible (p. 359). Although we are repeatedly and justly reminded that much of the geographic and scientific material in Strabo may derive not from the *Histories* but from the treatise *On the Ocean*, that hesitation is constantly ignored in Malitz's assertions of the philosophic character of the *Histories*. It is indicative of the quality of Malitz's arguments that the texts most emphasized were not printed by Jacoby, and Malitz goes beyond even Theiler in the discovery of ever less likely pieces of "Posidonius."

Scholarly scruples prevent Malitz from concealing the absence of evidence for his hypotheses about Posidonius's cosmic sympathies and didacticism, but he is undeterred. The "central theme of sympathy between heaven and earth" (p. 415)—defended, following K. Reinhardt, on the basis of a nearly incomprehensible sentence of Diodorus—was, Malitz admits, unrecognized by ancient readers (p. 416). The place of the *Histories* in Posidonius's philosophical *oeuvre* "is nowhere demonstrated by a fragment with his name" (p. 412). If there were such fragments, Malitz's task would have been easy and his conclusions plausible. But we are almost completely ignorant about Posidonius's historiography, and no amount of ingenious hypothesis can alter that fact.

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RICHARD A. BAUMAN. *Lawyers in Roman Republican Politics: A Study of the Roman Jurists in Their Political Setting, 316–82 B.C.* (Münchener Beiträge zur Papyrusforschung und Antiken Rechtsgeschichte, number 75.) Munich: C. H. Beck. 1983. Pp. xxii, 453. DM 145.

Richard A. Bauman's newest work meets those demanding standards of high scholarship and imaginative insight established by his own previous publications. This is a rich, learned, and intricately argued book. Bauman ranks with an elite group of international scholars who can boast expertise in both Roman public law and Roman history.

Erudition, however, does not make for easy reading. A firmer editorial hand would have been welcome. Doxographical material burdens the text. Bauman's scrupulous thoroughness drove him to consider rival theories on countless matters, great and small, often fragmenting his presentation and obscuring his own positions. The full analysis of evidence and fair rehearsal of alternative views make this a work that invites consultation on details—but discourages sustained reading.

Fragmentation, in fact, is a problem throughout. The title suggests a central theme, but the book fails to provide it. One anticipates a study of juridical learning employed in political circumstances. Instead, Bauman examines the political careers of select Roman *principes* who happen to be jurists. The influence of their legal skills on the public scene is often assumed, rarely discussed, and generally irrelevant. Bauman concentrates on a dozen major figures from Ap. Claudius Caecus to Q. Mucius Scaevola Pontifex, who played significant roles on the political stage. But their juristic interests surface only occasionally (and conjecturally) in relation to their public activities. Indeed, certain individuals who combined political involvement with prime reputations as lawyers—like M'. Manilius, M. Junius Brutus, Q. Aelius Tubero, and C. Marcus Figulus—are omitted by Bauman because their political careers were relatively inconsequential. Politics, rather than legal expertise, dictated the selection.

Bauman's study, as a consequence, moves from person to person and topic to topic without thematic unity. He treats numerous subjects of high political significance but with little connection to the juridical talents of the participants: for example, Ap. Claudius's resistance to the plebeian leadership, the eastern policies of the Aelii, Cato's *inimicitiae*, models and precedents of the Gracchan agrarian law, the composition of the *Annales Maximi*, the political allegiance of Q. Scaevola Augur, the attitude of Q. Scaevola Pontifex toward Italian enfranchisement.

None of this detracts from the value of Bauman's contribution, only from coherence and continuity. The individual discussions, taken as set pieces, are generally challenging, controversial, and illuminating. Bauman tackles each problem with tenacity, subjects the evidence to rigorous scrutiny, explores various avenues of interpretation, and frequently offers a novel reconstruction. The hypotheses will not all win assent, and some fall into the category of the strained and far-fetched. The *forensis factio* that backed Ap. Claudius, so elaborately conceived, vanishes almost as swiftly as it surfaced. The re-creation of Sex. Aelius's *Tripertita* as propagating a Roman message of *concordia* to the Greeks demands faith. Bauman's idea that Cato devised a "*maiestas*" treaty with Aetolia has no basis in the texts. The supposed connection between Scaevola Augur and Marius is

overdone and undocumented. And the convoluted discussion of Rutilius Rufus's trial delivers confusion rather than enlightenment.

Disagreements apart, this work has the great merit of engaging the reader in problems once regarded as solved or insoluble. Whether one accepts Bauman's solutions, the combination of ingenuity and erudition has reopened important issues and will provoke salutary reassessment. The book needs to be taken in small doses—but it will provide ample nourishment.

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ALEXANDER DEMANDT. *Der Fall Roms: Die Auflösung des römischen Reiches im Urteil der Nachwelt*. Munich: C. H. Beck. 1984. Pp. 694. DM 148.

Of all the realms of history the most traveled has been the decline and fall of Rome. And from now on all tourists and students will be able to rely on this guide, a Baedeker of all the theories and all the theorists one might care to know (and a good many more besides). Coverage is the first and outstanding achievement of the work: approximately fifteen hundred titles in the bibliography, twenty-five hundred entries in the index. Two hundred ten factors have been adduced in theories of the decline, and all have been discussed by Alexander Demandt, all are entered in the index, and all are thoughtfully listed together on the very last page of the work.

This great mass of material is splendidly arranged and presented in two long chapters that together amount to four hundred fifty pages of the text (out of approximately six hundred). The first chapter deals with epochs of interpretation, the second with types; thus both diachronic and synchronic approaches are used. One can quickly discover what has been said and thought about the decline of Rome from Sallust and Jesus down to Gerhard Wirth's study of St. Severinus (1982). In short, Demandt has given us an admirable Baedeker.

There is a second aspect of the work, one that I find less admirable. There is no sense of increased understanding, no indication of advances in knowledge. Indeed, as the 210 factors pass in review the impression that remains is one of utter chaos. Nor would Demandt reject this. Quite the contrary, to judge from his fourth and concluding chapter, which develops the idea stated in its epigraph: "People prefer the explanation which is most satisfying to their minds and hearts" (p. 601). That is from Johann Christoph Friedrich von Schiller, but it sums up perfectly the relativism and cynicism that pervade this work.

But Schiller's thesis can only be maintained by focusing exclusively on *theory*. If, instead, the focus were turned toward *analysis*, then the picture would be very different. First of all, a great many names in the book would simply disappear. It is interesting, and perhaps even significant, that Walter Rathenau (pp. 378–79) as well as Adolf Hitler (pp. 390–91) gave racist explanations of Rome's decline, but that belongs to the history of Germany, not Rome; it is an episode in the history of an illusion, not a chapter in historical scholarship.

Second, one would recognize the important advances in knowledge that have in fact been made. For example, Demandt notes M. I. Rostovtzeff's theory of "the absorption of the educated classes by the masses" and categorizes it as a *kulturphilosophische Interpretation* (pp. 452–54). But this occurs only on the very last page of *The Social and Economic History of the Roman Empire* and is not at all a theme of that great work. What is a theme—and its central thesis—is the opposition between *honestiores* and *humiliores* and between city and country. It is supported by a powerful analysis and a mass of evidence, and his conclusions have provided the basis for subsequent work by Cardascia and P. D. A. Garnsey and other scholars. Rostovtzeff's theory of "absorption" reflected the ideology of a discouraged liberal; his arguments on basic oppositions in Roman society were the work of a great scholar. It is the scholarship that endures, and that is where the focus of historiography should be.

Another example: Demandt's account of Franz Altheim (pp. 483–85) does not mention his path-breaking use of anthropological evidence, something that gives his work permanent value. It is significant in this connection that the first edition of Altheim's work on the decline of Rome was dedicated to Heinrich Himmler (Demandt chooses to cite only the second edition, in which a quotation from St. Augustine appears instead), and yet Altheim nevertheless held chairs in Leipzig and East Berlin after 1945. Evidently, the Marxist masters of East Germany were able to distinguish between ideology and scholarship. So should we.

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MEDIEVAL

ROLF SPRANDEL. *Gesellschaft und Literatur im Mittelalter*. (Uni-Taschenbücher, number 1218.) Paderborn: Ferdinand Schöningh. 1982. Pp. 310. DM 24.80.

Producing authoritative surveys of large historical topics is rather a specialty of German historians and

publishers these days. In this ambitious successor to his *Verfassung und Gesellschaft* (1975), Rolf Sprandel tackles the daunting problem of the relationship between literature and society during the high and late Middle Ages. More than a *Studienbuch* in the ordinary sense, Sprandel's work is closer to a thoroughgoing functionalist interpretation of the place of literature in medieval society. Without altogether avoiding the oversimplifications of this approach, his compact, yet remarkably comprehensive, book goes far toward achieving one of its major aims: the creation of a framework within which to assess the social representativeness of individual literary works.

As defined in his introductory chapter, along with other terms of his enterprise, literature embraces all texts not immediately pragmatic in purpose (charters, for example, are not considered as literature). Viewing society as a complex of "relations-systems," Sprandel presents a model of the functions of literature as mirror and, in varying degrees, as maker of the three intersecting and interacting "mega-societies" of this period: ecclesiastical, courtly-aristocratic, and urban. His tripartite model reflects his own historical interests, particularly the study of mentalities, whose perspectives, and those of the newer study of "communications-history," dominate his highly synthetic interpretive scheme.

Giving roughly equal space to each of his "societies," Sprandel adopts, with appropriate variations, a similar pattern in their treatment. Each major section begins with a survey of changing institutional and social structures; there follows a discussion of centers and modes of communication, a treatment of the evolution of literary genres and their functions, and, finally, an account of the mentality of each society as mirrored in and molded by its literary forms. Encompassed here are attitudes and values pertaining to a wide range of subjects: the stages of life from childhood to old age and death, women and marriage, the social order and its rulers and divisions, modes of piety, and the understanding of nature. After a glimpse of crucial change in the sixteenth century, in which the role of humanism is emphasized and that of printing oddly neglected, the book concludes with an extensive, topically organized bibliography that is most useful, though not entirely up-to-date.

Not all readers will be happy with a conception of literature that sacrifices style and meaning to function or with a notion of mentality that seems at times indistinguishable from ideology. Sprandel would probably agree with Brian Stock that "as texts informed experience, men and women began to live texts." But the format of this book leaves too little room for exploring what many students of medieval literature and society would consider critical issues in their interaction. Sprandel's model works best for

courtly-aristocratic society. Here he develops most skillfully a dominant theme of this section and the succeeding one: the breaking of the ecclesiastical monopoly with the rise of vernacular literatures and the slow emergence of a distinctive literate class, whose first true representatives, in his view, were the Italian humanists and their followers.

Scholars and students alike, whatever their questions or reservations, should welcome this provocative and substantial work, which very much merits an English translation.

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DELNO C. WEST and SANDRA ZIMDARS-SWARTZ. *Joachim of Fiore: A Study in Spiritual Perception and History*. Bloomington: Indiana University Press. 1983. Pp. xv, 136. \$20.00.

This slim volume attempts to do two things: to introduce readers to Joachim and his works and to present an interpretation of several of those works from the perspective of his understanding of history. Chapter 1 provides a biography of Joachim and a brief examination of his writings. Chapter 2 discusses basic symbolic patterns in Joachim's works. Chapter 6 seeks to demonstrate Joachim's "sustaining influence," and this is followed by a survey of the current state of Joachim scholarship. These parts of the book are primarily the work of Delno C. West. Although underdocumented, the two introductory chapters are adequate; the last chapter and the analysis of Joachim scholarship are not. West has provided a list that suggests that everyone from Christopher Columbus to Carl Jung to the theoreticians of Manifest Destiny in America were more or less consciously influenced by Joachim. In addition to providing inadequate documentation, West has seemingly made the all-too-common error of labeling any apocalyptic ideas as Joachite. The bibliographical essay is confusing, imbalanced, and unrelated to the all-too-brief bibliography that follows it. For example, he praises Bernard McGinn's selections from the works of Joachim in McGinn's *Apocalyptic Spirituality* (1979) and discusses a 1980 *Speculum* article by E. Randolph Daniel, but these works are not cited in the bibliography. Furthermore, there is no list of editions and translations of the works of Joachim in the bibliography, although there are scattered citations throughout the book.

Chapters 3, 4, and 5 are the heart of the book; these are the work of the other author, Sandra Zimdars-Swartz. Chapter 3 deals with Joachim's use of the Latin Fathers. The author proves the obvious—that Joachim relied heavily on Augustine and Gregory the Great. And she lists citations of these and several other Fathers in the major works of

Joachim. Unfortunately, she does not list which of the Fathers' works Joachim cited, thus making her work of little value to scholars. Chapter 4, "The Trinity and History," is interesting and useful, especially because it includes the general plan in outline form of Joachim's four major commentaries. The fifth chapter is a rather technical analysis of the five great works of Christ.

The obvious problem with this book is that it contains two different approaches to Joachim written for two different audiences. The general material seems to be aimed at people with little or no previous knowledge of the Calabrian prophet; however, I would recommend to the uninitiated Marjorie Reeves's *Joachim of Fiore and the Prophetic Future* (1976) rather than the introductory material in this book. The central chapters are interesting and suggest that we can expect some significant writings from Zimdars-Swartz in the future. But the material needs more refining, and she should incorporate into her analysis more about the shorter works of Joachim.

I am not convinced from the evidence presented in this book that it is proper to refer to the works of Joachim as "the most comprehensive analysis of history in the Middle Ages" (p. 111) or to claim that "Joachim's greatest legacy was his historiography" (p. 110). In other words, I do not believe that the authors adequately demonstrate their thesis, and often I wondered if they had lost sight of it. I am disturbed that the photographs of the famous *figurae* are not accompanied with a legible redrawing and translations. Without these aids, the photographs are no more than a curiosity to the reader. In the chapter concerning the influence of Joachim there is a discussion of the Spiritual Franciscans but, curiously, only the briefest reference to Bonaventure, which states that he was "attracted to the mystical joachite interpretations of St. Francis" (p. 104).

Generally, this is a disappointing book, promising much and producing surprisingly little. It lacks focus and a clear sense of audience. The reader is taken with no transition from a general introduction to an exploration of several rather difficult and technical problems of Joachite exegesis. Even the bibliography is a disappointment and not nearly as useful as others recently published. This book illustrates the possible pitfalls of dual authorship.

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ROBERT C. PALMER. *The Whilton Dispute, 1264-1380: A Social-Legal Study of Dispute Settlement in Medieval England*. Princeton: Princeton University Press. 1984. Pp. xxii, 295. \$28.50.

Robert C. Palmer's new book is an instructive and innovative study of a series of interlocking lawsuits over the Northamptonshire manor of Whilton. The stage was set for the Whilton dispute in 1264 when William de Whelton tried to settle this manor on his second son, Nicholas. The litigation resulting from this attempted settlement continued for over a century and ultimately involved many men and women linked by descent or marriage to the families of both Nicholas and his wife, Joyce la Zouche. Palmer's analysis of this litigation—which he heroically refrains from likening to *Jarndyce v. Jarndyce* in *Bleak House*—is broader than an outline of the Whilton dispute might lead one to expect. Instead of simply chronicling a small episode in local history, his book serves as an introductory textbook on medieval common law, as an essay in the social analysis of litigation, and as a study of how some people used "a certain set of state institutions to settle disputes" (p. 4). As a textbook, Palmer's study is highly successful. While tracing and explicating the history of a single set of lawsuits, he finds appropriate occasions for introducing nonspecialists to many of the topics and problems treated in more conventional introductions to medieval legal history. These unusually lucid, well-informed discussions of writs, final concords, pleading, and many other matters are especially valuable because they are so well illustrated by eighty-one translated documents relating to the Whilton dispute.

As an exercise in the social history of law, Palmer's book, though useful, seems somewhat narrow in scope. In commenting on the relationship between law and "common morality" and on "patterns of activity and expectation" that were typical of common law litigants (p. 5), the author does not use nonlegal materials. Nor does he draw on anthropological studies of dispute settlement or touch directly on the issues that social historians have raised, for example, in analyzing the so-called crisis of feudalism or the long-term history of English kinship. Palmer deserves credit, however, for showing how individual medieval lawsuits can sometimes be linked together to form what legal anthropologists might call an "extended case" and for examining more closely than many other legal historians the familial and tenurial connections of medieval litigants.

Finally, although Palmer does not fully substantiate his plausible claim that the study of legal processes such as the Whilton dispute "can yield valuable conclusions about normal social structure and behavior" (p. 10), he makes some interesting remarks about the role of the common law both in promoting compromises between individual litigants and in fostering social cohesion within England as a whole (pp. 212, 220). Moreover, after drawing attention to a process by which the medieval common law be-

came increasingly disassociated from "commonly held ideas of what was morally just" (p. 5) and from "normal" modes of action" (p. 217), he suggests that by measuring "this gap between law and life" (p. 219) at different times between 1160 and 1540, he can distinguish several significantly different eras in the history of English law.

In its present, abbreviated form, this schema is not entirely convincing, partly because Palmer says far more about law than he does about social life and partly because his image of law as being sometimes separated from life is no more compelling than the one used by some (but hardly all) Marxists who locate law in society's superstructure. Nevertheless, because Palmer is so skillful in explicating even the most complex features of medieval English law, it will be interesting and enlightening to see how he will develop his broad view of English legal history in subsequent writings.

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MARY LYON *et al.* *The Wardrobe Book of William de Norwell, 12 July 1338 to 27 May 1340*. Assisted by JEAN DE STURLER. Brussels: Palais des Académies, for the Académie Royale de Belgique, Commission Royale d'Histoire. 1983. Pp. cxxiii, 546.

In 1920 the late T. F. Tout remarked in the first volume of his pioneering *Chapters in the Administrative History of Mediaeval England*, "No greater service could be performed for fourteenth-century history than the establishment of a society something on the lines of the Pipe Roll Society to make these invaluable records (wardrobe accounts) more easily accessible" (p. 48). This reviewer could still point to the lack of progress in this matter in a paper read at the Anglo-American Historical Conference in 1947. One reason for this is the incredible number of subsidiary documents that have survived, for example, Patricia M. Barnes's *List of Documents Relating to the Household and Wardrobe John-Edward I* (Public Record Office Handbooks, 7 [1964]). Editing a wardrobe account book is accordingly a Herculean task requiring the editors to repeat Barnes's performance for the time span of the document with which they are concerned, in this instance, 12–14 Edward III. Little wonder that only two wardrobe books have been published since John Topham's unsatisfactory edition of the *Liber quotidianus contrarotularis garderobae*, covering the years 1299–1300, published by the Society of Antiquaries of London in 1787. The first was the *Liber contrarotularis de necessariis expensis* for 1285–86, edited by B. J. Byerly and C. M. Byerly as *Records of the Wardrobe and Household, 1285–1286* (HMSO, 1977). The second is the present one, the first by a keeper, as opposed to

the duplicate kept by the controller, and it is best described by a translation of the complete Latin title: *Book of Details of the Account of William de Norwell, Keeper of the Wardrobe of King Edward, Third after the Conquest, Concerning the Same Wardrobe between 11 July 1338 and 28 May 1340*.

The introduction contains sections on the historical evolution and functions of the wardrobe and on the household and wardrobe under Edward III. They are a godsend to the uninitiated, for they constitute the best up-to-date short essay (60 pages) ever written on medieval English administrative development to the end of the fourteenth century. The third section (54 pages) deals with the contents of the manuscript itself and serves as a sort of précis of the 461-page text. It becomes obvious why this book is of crucial importance, for it is a gold mine of detailed information concerning the daily working of the English government at a crucial period in its history, the years immediately preceding the outbreak of the Hundred Years' War. This information includes receipts from all sources; household expenses and where they occurred; "foreign" expenses (alms, necessities, gifts, messengers, fees, robes, replacement of horses, wages of men-at-arms, archers, sailors, and passage of horses of magnates); jewels and vessels; and prests. In short, the book is a meticulous and detailed account of management, finance, war and diplomacy, and even the everyday life of king and court. It is easily consulted because of the clear titles throughout and an excellent index of persons and places.

Despite the kind words in the preface about the roles played by the late Henry S. Lucas and the late Jean de Sturler, this reviewer knows that it was really Mary Lyon and Bryce Lyon who did the work and deserve the credit for giving us this definitive edition, which is a model of impeccable scholarship.

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LAURENT THEIS. *L'Avènement d'Hugues Capet, 3 Juillet 987*. (Trente Journées Qui Ont Fait la France, number 4.) Paris: Gallimard. 1984. Pp. 311. 98 fr.

This book is a fine example of the *haute vulgarisation* of medieval history that in recent years has flourished in France. Based on solid research and current in perspective, it will appeal to the historically literate French public and, in the freshness of its overview and synthesis, to scholars as well. Genealogical tables, translated extracts from the sources, a series of illustrations, and a bibliography complement the text. Unfortunately, there are no notes.

The material is first-rate, and Laurent Theis handles it effectively. He takes a "non-event"—the accession of Hugh Capet as king of the West Franks—

situates it in the evolution of the political society in which it occurred, and shows how only in the eyes of later generations did this episode become a turning point in French history. Although the presentation contains insights and theories that will be stimulating for specialists in the field, its chief impact will probably be on the wider audience that one may anticipate for the first survey of tenth-century French royal politics to appear in decades. In particular, Theis's depiction of the governance of the kingdom—its fragmentation into principalities, the regional princes' determination simultaneously to preserve the royalty yet to keep the monarch weak, the effective subordination of the West Frankish rulers to the Saxon emperors—strikes telling blows against the old nationalistic historiography, whose anachronistic premises continue to influence popular, and even some scholarly, views of the period. The nationalist elements figure here only as part of Hugh's legacy—in Theis's repetition of Ferdinand Lot's hypothesis that the establishment of non-Carolingian rule in the Western kingdom produced a clearer differentiation between what would become France and the Germanic empire.

As is inevitable in a book of this scope, the individual reader may find certain emphases that he questions or topics that he thinks require fuller treatment than Theis gives them. For example, Theis seems to this reviewer overly trusting of Richer's portrayal of Hugh Capet and his ancestors, in which (following Richer's legitimist bias) a number of untrue or dubious statements appear to be contrived to cast aspersions on Hugh's suitability as king. Among the subjects inadequately explored are the high status and active political role accorded to the women of the royal and princely families during the middle and later tenth century. The popular audience might want a more detailed description of Hugh Capet's material resources and of the system of alliances on which he relied. Scholars will note that the editions of Gerbert and Orderic Vitalis used are not the most recent ones and that the bibliography lists only the Paris manuscript of the *Karolinus* of Giles of Paris, not the printed critical edition.

In sum, the criticisms of the book are very largely a corollary of its merits—that is, Theis has given us so much that is good that one wishes for more.

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DIETRICH LOHRMANN. *Kirchengut im nördlichen Frankreich: Besitz, Verfassung und Wirtschaft im Spiegel der Papstprivilegien des 11.–12. Jahrhunderts*. (Pariser Historische Studien, number 20.) Bonn: Ludwig Röhrscheid. 1983. Pp. 375. DM 98.

Papal confirmations of ecclesiastical holdings listing the possessions of individual religious houses contain much valuable information for our understanding of numerous aspects of medieval life. In this slightly revised version of a dissertation presented in 1978 to the Johannes-Gutenberg-Universität in Mainz, Dietrich Lohrmann demonstrates the truth of this statement for the churches of northern France in the post-Gregorian world of the late eleventh and twelfth centuries. The introduction to the work accurately places the study into the historiographical tradition by acknowledging the author's debt to earlier scholars such as Emile Lesne and Georg Schreiber, to more recent contributors such as Jacques Dubois and Robert Fossier, but most especially to Johannes Ramackers, whose multivolume edition of papal documents for France serves as the basis for a substantial portion of Lohrmann's study.

The body of the book is divided into three segments of which the second is the longest and most valuable. Part 1, the briefest and least satisfactory portion, offers a cursory survey of the listing of ecclesiastical possessions in northern France from the eighth century and a consideration of the nature of a papal confirmation. The second part examines the possessions and income of individual churches, including the wealth of the bishops, canons, monks, and other religious of the ecclesiastical provinces of Rouen, Reims, and Sens. In the final part Lohrmann seeks to show the value to historians of these lists of goods. He notes in particular their utility for cartography, for providing information on the provenance of the possessions, and for supplying economic data on the forms of production and variety of labor. There is a brief conclusion, a short postscript, and a number of plates reproducing papal confirmations or requests for these privileges. The plates are particularly interesting when one compares the differences between a request and the resulting papal response.

In a work with such a wide scope it is not surprising that there are weaknesses. More analysis of individual confirmations might have lessened the catalogue nature of some of the sections. The work would also have benefited from more comparisons of the holdings of the individual houses. Although Lohrmann supplies a number of maps to illustrate in detail ecclesiastical holdings in limited areas, he fails to offer general maps of all of northern France to demonstrate his findings.

Yet what he has provided is a rich survey that will be of use to a wide variety of historians. The author is aware of how much more can be done with these documents and suggests many areas for further research. The general excellence of this study leads

one to hope and expect that Lohrmann will fill some of these lacunae himself.

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ANNE-MARIE LEGRAS. *Les commanderies des Templiers et des Hospitaliers de Saint-Jean de Jérusalem en Saintonge et en Aunis*. Paris: Centre National de la Recherche Scientifique. 1983. Pp. 216. 60 fr.

The numerous European estates of the military orders provided their convents in the Near East with funds and recruits and thereby were essential to them. The Templars, who in France alone had probably three times as many properties as the Hospitalers, were particularly wealthy, and the Hospitalers, who took possession of most of the Templar lands after the suppression of the Temple in 1312, became members of one of the best-endowed orders in Christendom, holding on to much of their property in Catholic Europe until the eighteenth century. The basic unit of administration in both orders was the commandery, with its outlying houses and granges. Over the centuries the pattern of commanderies in a given region was subject to constant modification, which testifies to the care the orders took with estate management.

The interest of younger historians of the military orders appears to be shifting from the Latin East to Europe, and the best of them are setting standards that other scholars will have to match. Michael Gervers's treatment of the Hospitalers' estates in Essex (*The Hospitaller Cartulary in the British Library* [1981] and *The Cartulary of the Knights of St. John of Jerusalem in England: Secunda Camera Essex* [1982]) is now joined by Anne-Marie Legras's examination of the properties of the Templars and Hospitalers in one region of western France, the old diocese of Saintes. This is the first in a series of volumes planned to include detailed studies of all of the French commanderies, so, although it is valuable in itself, its value will increase as other volumes appear and a general picture becomes discernible.

In this work the historical, archaeological, and architectural evidence is brought together; this, incidentally, makes it an important bibliographical guide. The introduction is excellent on the history of the management of the French estates but is a little shaky on the general history of the military orders. One wonders, for example, what the Holy See and the Grand Master of the Order of Malta in Rome will make of the remark that in 1798 the Order of St. John of Jerusalem "ceased to exist in its original form" (p. 12). The author treats each commandery, together with its dependent houses, in turn, and, as might be expected, her descriptions show how valuable the early modern and nine-

teenth-century evidence, particularly that provided by the visitation records, is for an understanding of the medieval structures that still stand today. She also demonstrates how a study of the existing buildings can supplement the documents: very few charters issued earlier than 1200 survive, but in the majority of the commanderies some buildings were constructed in the twelfth century, pushing back the dates of their foundation by nearly fifty years.

It would have been nice to have had more discussion of parish churches over which the orders had rights of patronage. In England there are often clear signs of the orders' influence in the architecture and decoration of these churches, and the same must be true of France. When she describes such churches at Chepniers, Courant, and Thairé, however, the author seems to be interested in them only in the context of the brothers' conventual life. But that is a small criticism. This is an important book, and all historians interested in the military orders will have to consult it.

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ALEXANDER CALLANDER MURRAY. *Germanic Kinship Structure*. (Studies in Law and Society in Antiquity and the Early Middle Ages, Studies and Texts, number 65.) Toronto: Pontifical Institute of Medieval Studies. 1983. Pp. xii, 256. \$21.00.

This book, difficult to read, wordy, and repetitious, is still well worth the effort, because it convincingly challenges some long-cherished beliefs of medievalists regarding the structure of early Germanic society and law. Primary emphasis is on early Germanic kinship, but there are interesting discussions on law, association and social organization, family institutions, and the German and Roman heritage of the early Middle Ages.

Part 1 first summarizes and then evaluates on the basis of modern anthropological methodology the major theories of Germanic kinship structure and development. Alexander Callander Murray finds no evidence for the oft-repeated generalization that early Germanic society was based on a clan or extensive lineage structure. And, contrary to previous scholarly conclusions, he sees kinship structure not based exclusively on agnation (relationship through males) or exclusively on matriliney (relationship through females), but on cognation (relationship through both males and females). Challenged also is the famous *Markgenossenschaft* theory held dear by all Germanist historians since the late eighteenth century.

Part 2 minutely examines the evidence to substantiate the author's conclusions. One by one, the

famous sources are questioned either as to authenticity or as to interpretation by scholars about early Germanic society. Found completely unreliable is Julius Caesar's *Bellum Gallicum*. Caesar knew little about the Germans and derived most of what he said from the earlier ethnographical ideas of Posidonius on the Celts. If Murray is correct, we must bid adieu to the classic teaching that the Germans of the first century B.C. were nomadic and pastoral. The *Germania* of Tacitus, though more reliable, offers no evidence of the clan as a basic unit of society, a conclusion at odds with scholarly consensus. It also "indicates a kinship system based upon bilateral kindreds and thoroughly imbued with the cognatic principle" (p. 64). Buttredding his emerging thesis with evidence on the *vicini* provided by the *Pactus Legis Salicae* and the *Edictum Chilperici* as well as with that learned from the *fara* and *genealogia*, Murray states, "The clan as a political division, a primitive economic association, an agent of settlement in the successor kingdoms and a military unit fades away before a critical examination of the sources which have long been thought to lend support to its existence" (p. 109).

Part 3, of special value to legal historians, probes the *Lex Salica* for information concerning kinship, the bilateral kindred, feud and compensation, oath helping, and inheritance. What emerges is that, when the Salic Law was first compiled in the early sixth century, the Franks practiced a system of individual or family ownership. The law was not concerned with corporate groups and their ownership or possession but with categories of kinsmen who as individuals assumed possession of land and chattels from deceased relatives. To accept that this was the intent of Salic law makes the theory of the *Markgenossenschaft* even more ridiculous and discredits the dictum that there was a long-term transformation of an original Germanic collective society to an individual one, a process completed by the time of Charlemagne.

Most of the arguments and textual interpretations are convincing, leading to the conclusion that most of what historians have written on early medieval society has been based on assumptions about primitive societies rather than on the evidence of pertinent sources. A fresh work full of challenges for those concerned with the early Middle Ages, this book will force revision of much of the writing about the early Germans, even that of Marc Bloch and of Marxist historians.

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ROBERTO RUSCONI. *L'Attesa della fine: Crisi della società, profezia ed Apocalisse in Italia al tempo del grande scisma d'Occidente, 1378-1417*. (Istituto Storico Italiano per

il Medio Evo, Studi Storici, numbers 115-18.) Rome: The Institute. 1979. Pp. 279.

Waiting for the end and speculating about when and how it would come was, as Roberto Rusconi points out at the beginning of his learned and valuable book, a general medieval preoccupation. Many people, both clerics and laymen, tried to recognize the signs described in the Book of Revelation and other prophetic parts of the Bible and watch for manifestations of the single or multiple antichrists about whom so many theologians and pamphleteers had written. Eschatological (referring in general to the last things) and apocalyptic (denoting, according to Dupré Theseider and Rusconi, an effort to unlock the secrets of St. John's Apocalypse) are adjectives that appropriately describe a vast amount of medieval literature, more of it orthodox than heretical. As long as the validity of basic ecclesiastical institutions was not denied, the church allowed its members a great deal of latitude in predicting the nature and date of attacks on it by enemies within and without. Some Christians envisaged a period of continuing tribulation for the church until the final paroxysm of the Antichrist's coming, his overthrow after a fierce but brief persecution, and then the Last Judgment. Others foretold the dawning of a virtuous and happy age either before or after the Antichrist, though before the Last Judgment. Rusconi, following Robert Lerner, says that royal and imperial publicists were more likely to postulate a golden period before the Antichrist that would be presided over by one or more great rulers, whereas ecclesiastical authors, some but not all of whom were influenced by Joachim of Fiore, were inclined to put the Antichrist's appearance first and emphasize the purification of the church that was supposed to result from his persecutions.

Rusconi regards some previous work on prophecy, ranging over large stretches of time, as too theoretical, too much the depiction of a battle of the books divorced from social and political contexts. He tries to show how particular interests and events shaped views of the final age. At the same time Rusconi is not satisfied by global psychological, social, or political explanations. He rejects, for example, Norman Cohn's highly colored view of apocalypticism as a reflection of mass hysteria and rebellious anarchy. He also declines to endorse the Marxist theory that apocalypticism expresses a pre-political negativism in regard to existing society on the part of the lower classes. He thinks instead that the variety of eschatological theories mirrors the efforts of different individuals and groups to come to terms with their own times, using for this purpose insights derived from the Bible, still their main ideological guide. The resulting theories and atti-

tudes might sometimes have stimulated feverish activity, as with an individual like St. Bridget of Sweden or a group like the Taborites. In the time (1378–1417) and the place (the Italian peninsula) that Rusconi studies, however, the main characteristic he finds in eschatological imaginings is pessimistic passivity, largely caused by the unnerving spectacle of the Great Schism. It is somewhat ironic that, in the period and region Rusconi has chosen, his method of linking theory and practice and taking careful account of diversities in background and purpose only seems to underscore the relatively monotonous manner in which Italians in the late fourteenth century viewed historical events. But Rusconi's method is sound, and his book is an erudite and painstaking survey of an important subject.

CHARLES T. DAVIS
Tulane University

APOSTOLOS KARPOZILOS. *Symvoli sti meleti tou viou kai tou ergou tou Ioanni Mauropodos* [Contribution to the Study of the Life and Work of Ioannis Mauropous]. (Annals of the School of Philosophy, number 18.) Ioannina: University of Ioannina. 1982. Pp. 191.

This work is a printed version of a mimeographed private publication that first appeared in 1979 with limited circulation to university and college libraries. The Dumbarton Oaks Center for Byzantine Studies, for example, does not own a copy of the original. In a sense, the current publication is an improvement on the earlier work and is therefore a new edition, incorporating the comments and criticisms made in the reviews of P. K. Christos (*Klironomia*, 12 [1980]: 441–44), P. Gautier (*Revue des Études Byzantines*, 38 [1980]: 310), Francois Halkin (*Analecta Bollandiana*, 98 [1980]: 434), and Wolfram Hörander (*Jahrbuch der Österreichischen Byzantinistik*, 30 [1981]: 367–68). Apostolos Karpozilos, however, overlooked the incisive review by Daniel Sahas, which appeared in *The Greek Orthodox Theological Review* (26 [1981]: 250–52) but, admittedly, might not have been available to him prior to republication.

Karpozilos's study of John Mauropous is a serious scholarly work, carefully researched, well reasoned, and skillfully written, although the book is not without controversial aspects that will necessitate further scholarly discussion and disputation. The book is divided into two sections: the first deals with the life of Mauropous and the second with his works (that is, epigrams, letters, and addresses). The second section constitutes by far the bulk of the book.

Little is known of the personal life and many of the activities of John Mauropous, a significant eleventh-century figure who was a teacher at the university in Constantinople and then metropolitan of

Euchaita in Asia Minor. Karpozilos has quite successfully extracted nearly every shred of evidence from all available sources to reconstruct the life of Mauropous. He portrays Mauropous as a humane and highly moral person, whose interests moved from poetry to scholarship to monastic life. The space limitations of this review do not permit a critical evaluation of specific details and of Karpozilos's own interpretation of them. Later studies will have to reevaluate his conclusions with regard to Mauropous's place and date of birth, his family history, professional and religious associations, his influence on the programs of the university, his teaching career, and his influence on illustrious students such as Michael Pselleus and John Xiphilinus. But even this brief life (pp. 23–50) sheds new light on its subject and raises other questions and issues that will stimulate scholarly interest.

The second part, which is obviously of greater significance, is a meticulous critical study of extant manuscripts. Karpozilos's methodology is consistent throughout. He provides descriptions of texts, including the date of composition, to whom, for whom, and for what purpose they were prepared, evaluates their contents, and examines the contradictory conclusions about the manuscripts in modern scholarly literature. The author does provide a good summary of contemporary questions, issues, and interpretations of Mauropous's writings.

Karpozilos has produced the first, though not the definitive, monograph on a little-known Byzantine political, literary, and religious figure. His pioneering effort should encourage further scholarship in all aspects of Mauropous's life and works.

WALTER K. HANAK
Shepherd College

HENRIK BIRNBAUM and MICHAEL S. FLIER, editors. *Medieval Russian Culture*. (California Slavic Studies, number 12.) Berkeley and Los Angeles: University of California Press. 1984. Pp. x, 395. \$35.00.

Henrik Birnbaum and Michael S. Flier have compiled a remarkable variety of articles pertaining to the culture of the East Slavic world from the ninth through the seventeenth centuries. An international brigade of scholars contributed to this volume of essays intended to raise problems for future research and to address major interpretive issues in our approach to medieval Russian culture. Those essays not originally in English were translated by an accomplished, if not always consistent, editorial staff, so that the miscellany under review can be read in its entirety by scholars not familiar with Slavic languages.

The volume is divided into three discrete parts: "Culture and Society," "Art and Architecture," and

"Language and Literature." Contributors to part 1 consist of Birnbaum, Flier, Norman Ingham, Russell Zguta, Andrzej Poppe, and Iakov S. Luria. Part 2 has articles by Konrad Onash and Nikolai Dejevsky. The third part includes essays by Dean Worth, Riccardo Picchio, Ralph Bogert, Jostein Børtnes, Gerd Freidhof, and Boris Uspensky.

For the scholar of medieval Slavic culture this volume is an enormous convenience. He or she can readily observe recent trends in philological, linguistic, and art-historical study in some depth, and intriguing remarks by Luria on late fifteenth-century ideology and Zguta on monastic medicine are included as well. This reviewer must wonder, however, about the utility of stringing together a collection of excellent articles on mostly highly specialized and unrelated issues. If this work is to explain to the non-Slavic world what is happening in the field of medieval Russian culture, it fails in that it is too esoteric. If its aim is to provide convenient access to scholars—especially Slavic linguists—in the English-speaking world, it succeeds admirably but hardly seems necessary. Save perhaps Norwegian, we all read comfortably the languages of the European world.

This reviewer is gratified to see in the same volume the writings of scholars from the Soviet and non-Soviet worlds about early Russian culture and to note the dedication of the volume to the Soviet scholar D. S. Likhachev. The lack of structure and coherence, however, and the limited utility of the collection to any but specialists in the field lead to concern about why such a potpourri was so packaged. The big picture is missing, and it might well have been provided had there been comprehensive essays on medieval Russian culture (and society) or a common theme other than the amorphous subject of culture across all of Eastern Europe (not just Russia) over a thousand years of history.

Nevertheless, *Medieval Russian Culture* is a treasure house of scholarly gems that will provide many specialists and future specialists with ideas for new research. It is a must for our libraries.

ELLEN S. HURWITZ
Lafayette College

MODERN EUROPE

WILLIAM MONTER. *Ritual, Myth, and Magic in Early Modern Europe*. Athens: Ohio University Press. 1983. Pp. 184. \$24.95.

The only thing wrong with this work is the title, for this is a history of toleration in early modern Europe. The discrepancy is unfortunate, for William Monter's book is an eminently learned and fair

study of a very important subject, and it deserves a public that it will probably not now get.

The book, both a social and an intellectual history, is broadly revisionist. The structuring argument is that toleration came later than normally assumed and that superstition persisted longer—well into the age of Edward Gibbon. The author argues, for example, that the enlightened despot Joseph II, as regards the Jews, parallels the unenlightened Counter Reformation pope, Paul IV. On the Iberian peninsula, Judaizers still suffered persecution well into the eighteenth century. Generally, more witches were killed in the "modern" seventeenth century than in our benighted Middle Ages. It was in the seventeenth century that the worst religious massacres occurred, interestingly, at the extremities of Europe: this is, in Ireland and in Eastern Europe, whether carried out by Cromwell and his heirs or by the cossacks. England comes off rather poorly in the period, with the author quoting Henry Kamen to the effect that the Toleration Act of 1689 was in fact "reactionary in tone and content" (p. 169). And I can hardly resent corroboration of my own work (*Masts of Satan: The Demonic in History* [1983]), specifically in that the "vast majority of the 500 witch trials preserved from medieval Europe occurred after 1425" (p. 19) and to the effect that the Mediterranean inquisitions come off much better in this respect than their northern equivalents (p. 67). Toleration was eventually achieved less by the Scientific Revolution than by the more "silent revolution" of Cartesian rationalism (pp. 115, 127). Of course, the French Revolution recast the problem, only to replace the stake with the guillotine—as I am compelled to add. Most of the above, especially the effectively inverted appreciation of medieval versus early modern persecution, has been accepted by specialists for some time. The main value of the work is in synthesis and impartiality.

This hardly does justice to the sweep of the work. It contains interesting and informed sketches of characters as diverse as Erasmus and Sabbatai Zevi or Pierre Bayle and Spinoza. I also detect some incidental lapses. I am not sure that I would speak of Bayle's "philosophical defence of atheism" (p. 119) or allow that "all Christian martyrdoms before the third century were the result of popular rioting rather than state policy" (p. 3). The travail of liberty is relieved, I sometimes suspect, by humor. For example, Monter remarks on the "word-magic" (p. 32) in the Mass and the Protestants who "celebrated a ritual peculiar to Reformed Protestantism": a "day of community fasting and prayer" (p. 46). But these examples only underscore that the work is fair. Let us hope Monter's message is assimilated.

CHRISTOPHER DONALD NUGENT
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BRIDGET HILL. *Eighteenth-Century Women: An Anthology*. Boston: George Allen and Unwin. 1984. Pp. ix, 271. \$29.95.

Reviewing an anthology is a difficult and thankless task. Bridget Hill herself points to the source of the difficulty when she begins her book with the words, "Anthologies need some justification" (p. 1). She attempts to justify her effort by noting—correctly—that the history of English women during the eighteenth century has been slighted, particularly when compared with the extensive and illuminating studies available on women in the nineteenth century and the promising beginnings of solid work on the seventeenth. Her explanations of this lacuna are ingenious and, on the whole, convincing, and she is generous in pointing out that American scholars have been more active in investigating the varied experiences of women in eighteenth-century England than their British counterparts. She never really explains, however, why she has chosen to use the format of an anthology rather than to shape her material into a much-needed study of the history of eighteenth-century Englishwomen. Thus, the purpose of the book remains somewhat cloudy. The introductions to the book as a whole and to each of her thirteen parts are not substantial enough to add measurably to the knowledge of the specialist or to provide a convincing interpretation of the broader subject, and the format does not lend itself to the needs of the more general reader.

This said, however, it must be admitted that Hill has provided a splendid anthology. It is comprehensive and well organized. The range of materials used demonstrates a prodigious effort in uncovering illuminating insights into women's experiences from unlikely as well as from obvious sources. The bibliography of primary sources makes it an essential tool for any serious student of the subject.

In those sections dealing with subjects that have received more attention in recent years—"Ideas of Female Perfection," "Chastity," "Female Education," "Approaching Marriage," "Marriage and After," "Women's Legal Position," "Women Protest"—the selections, especially those dealing with the upper and middle classes, are somewhat predictable. Less familiar, and thus more interesting, is the material Hill uses to uncover the experiences of women of the lower orders, a much more difficult task. Here she shows her ingenuity and makes her greatest contribution. Particularly valuable are her sections on "Women without Husbands," "Crime and Punishment," "The Female Poor," and "Women in Agriculture." Her section on "Female Domestic Servants" and her suggestion in the introduction on the importance of domestic service in shaping the attitudes and experiences of both the servants and their mistresses are intriguing but need further elabora-

tion and interpretation. Her repeated assertion that eighteenth-century Englishwomen of all classes lost status because changes in the society and the economy reduced their productive contribution, while not unfamiliar, needs further study. It is always dangerous to accept the evidence of social critics who see moral decay in the increased "luxury" enjoyed by the newly prosperous. Likewise, recent studies of the agricultural changes of the eighteenth century suggest that enclosures did not decrease employment. Is it thus certain that women, however poorly paid, saw a decline in their opportunities for agricultural labor?

JEAN E. HUNTER
Duquesne University

GERHARD A. RITTER. *Sozialversicherung in Deutschland und England: Entstehung und Grundzüge im Vergleich*. (Beck'sche Elementarbücher; Arbeitsbücher: Sozialgeschichte und Soziale Bewegung.) Munich: C. H. Beck. 1983. Pp. 188.

In this slim volume, Gerhard A. Ritter has expanded a lecture delivered before the Max Planck Institute into a provocative discussion of the social-insurance legislation enacted in imperial Germany and Edwardian Britain. Like most scholars, Ritter views the social-insurance legislation of the 1880s as an attempt by Bismarck to win workers away from social democracy; thus, its origins cannot be separated from the repressive antisocialist law. He goes beyond this old verity, however, to view Bismarck's policies as an attempt to transform the financial burdens of pauperism from the local to the national level, thereby strengthening the strapped conservative rural communes. Ritter also deftly links the social legislation to Bismarck's efforts to put Germany's finances on a sound basis and to enhance the competitive character of German industry by creating a healthier and more productive work force (a motive that was also present in Britain). He also points to the glaring omissions in prewar welfare legislation, which included no limitation of the hours worked by adult males, no national unemployment insurance, and little recognition of the appalling situation of working-class widows.

On balance, Ritter's evaluation of the effects (some of which were unintended) of Germany's first steps toward the welfare state is quite positive. Since the insured played a major role in administering the health insurance system, more than three thousand free union members sat on the paid boards of the *Ortskrankenkassen*. Both the health and independence of the workers from employer control benefited measurably from the new insurance schemes. An unforeseen effect was a rapid per capita rise in doctors and dentists. Pension funds also eased the

housing crisis by investing in over three hundred thousand new housing units for workers. By 1914 clerical workers, servants, and agricultural laborers were added to the compulsory insurance system.

Ritter finds the British insurance legislation of the Lloyd George period more generous in its pensions, more comprehensive because it sought to deal with unemployment, and more compassionate than the German because it did not ignore widows. He also sees the British tax system, unlike the German, as leading to a redistribution of income, because it was based on direct taxation. An unexpected irony is that the German system allowed for more self-regulation by the insured than did the state-administered British system.

Ritter's analytical discussion of Bismarck's social legislation is the most incisive I have read. It is full of insights and suggestive of fruitful avenues for further research. I do have some reservations about his rather glowing discussion of Britain. If the British took the lead in unemployment insurance, it was a result of their serious unemployment problem—one that imperial Germany did not share. Pensions were, no doubt, higher in Britain because of that nation's greater wealth and the higher weekly wages paid to British workers. By 1914 imperial Germany spent a greater percentage of its net domestic product on social welfare than did Britain, France, and the U.S. combined. Despite direct taxes, free trade, and the Lloyd George social program, inequality of income was far greater in prewar Britain than in the Kaiser's Germany. Although there is overwhelming evidence to the contrary, German historians, including Ritter, continue to see a connection between parliamentary government and the mitigation of social and economic inequalities. With this caveat, I recommend Ritter's superb analysis to all modern European scholars.

KENNETH BARKIN
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NICHOLAS ROSTOW. *Anglo-French Relations, 1934–36*. New York: St. Martin's. 1984. Pp. xii, 314. \$25.00.

It is an established fact of diplomatic history that Britain and France approached post-World War I European affairs, especially in relation to Germany, from vastly different perspectives and that the breakdown in Anglo-French cooperation significantly increased the fragility of peace. That fact is strongly reinforced in this study, which dissects a crucial time segment in the Anglo-French interwar relationship, demonstrating that both governments, but especially the British, found it impossible to accept the truth of bad news and to act on it and suggesting some reasons why that was so. It is a

distressing story, centering on the conflict among rival theories about the ends and means of foreign policy both within London and Paris and between them. It leaves the reader limp from overexposure to confusion, contradiction, timidity, manipulation, illusion, and shortsightedness.

Impressively researched and documented, the book is sharp in organization and interpretation. Analyses of British and French groping for a policy during the latter part of 1934 are followed by chapters on the London Conference (February 1935), Stresa, the Franco-Soviet alliance and the Anglo-German naval agreement, Ethiopia, and the Rhineland crisis. It is clear throughout that the national interest of both nations required an intimate relationship. But it was not achieved. The Anglo-French connection was enormously complex, and passions and prejudice had as much to do with crucial decisions as calculations of national interest. Personalities played a significant role, and Nicholas Rostow examines the thinking of leading figures on both sides. Torn by contradictory impulses, many of them practiced fantasy and self-deception and got caught in a vicious circle from which there was no escape: Paris, conscious of the need for Britain's support, would act according to what London did, and London, suspicious of intimate relations with France, would act only on the basis of what Paris decided to do. The so-called Stresa front is depicted as a sham, the Anglo-German naval agreement as a fundamental British failure of vision that poisoned relations with France, and the Ethiopian crisis as a major milestone on the road to war because it revealed a basic conflict in the principles of Anglo-French foreign policy formulation. The Rhineland episode exposed contradictions in British policy: Hitler's acts made it impossible to trust him, but the British thought it in their interest to conclude a settlement with him while he was in the mood. France perceived the dangers but was not about to act without complete assurance of Britain's full support, which was not, of course, forthcoming. And the significance of it all lay in the fact that the contours of Anglo-French relations in the middle 1930s set the pattern for the rest of the decade, rendering them impotent in the face of later fascist aggression.

After the Great War, the *idea* of Entente remained but the reality did not. In that segment of its demise presented here, the French come off somewhat better than the British. Reliance on cooperation with Britain was the cornerstone of French diplomacy, and Paris tried hard to entice the British into alliance. But the British, influenced by both geography and history, believed that they had a wide choice of foreign policies and refused to commit themselves to France. This strengthened French indecision and inertia and undermined morale and

will. British politicians produced diverse and inconsistent opinions about foreign policy, but the majority hoped to pacify France in order to pursue agreement with Germany. They failed to grasp the connection between European security and the dominance of democratic forces in European culture, which the French viewed as a betrayal of responsibility. French governments ultimately deferred to British wishes in diplomatic problems involving the Anglo-French relationship so as to avoid having to deal alone with Germany. The British were loathe to accept unlimited responsibility for Europe's future. These two preoccupations, the author believes, explain the gradual loss of Anglo-French ability to control events and protect their destinies.

This study has important interpretive implications, especially in its challenge to those historians who have come to believe that British and French diplomats of the appeasement era, who were once subject to excoriation, should now be rehabilitated because scholars have discovered how narrow were the policy choices open to them. It suggests convincingly the unlikelihood that even different economic conditions would have substantially affected military and diplomatic decisions. And it is a short step back to the older view that, in many ways, the appeasers simply "blew it."

WILLIAM R. ROCK
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RICHARD SCHLATTER, editor. *Recent Views on British History: Essays on Historical Writing since 1966*. New Brunswick, N.J.: Rutgers University Press. 1984. Pp. xiii, 525. \$50.00.

In 1966 the Conference on British Studies sponsored a collection of essays that reviewed the progress of British historical studies between 1939 and 1960. The present volume continues the investigation down to approximately 1978, although several contributors run on by a year or two. Three scholars who worked for the earlier book have nobly stayed at the helm: Bryce Lyon (medieval history to 1307), Henry Winkler (the twentieth century), and Maurice Lee, Jr. (Scotland). The remainder are new: Barbara A. Hanawalt (the later Middle Ages), Wallace MacCaffrey (the Tudors), David Underdown (1603–60), Stephen B. Baxter (1660–1714), Henry L. Snyder (1714–60), Robert A. Smith (1760–1820), D. C. Moore (1820–70), Peter Stansky (1870–1914), L. P. Curtis, Jr. (Ireland), and Robin W. Winks (empire and commonwealth). The balance has also shifted against all history before the eighteenth century, a fact that reflects editorial choices quite as much as actual productivity; the later contributions cover much shorter periods

much more lavishly. Left to their own devices, contributors have adopted notably differing methods of approach. Lyon laments the alleged decline of medieval studies but ignores the headstart that sector has had in serious historiography. Hanawalt, MacCaffrey, and Lee present classified lists but minimize personal comment; Underdown, however, wishes to sing the praises of social history and to demonstrate his respect for some well-established practitioners. Baxter prefers the role of the hanging judge, while Snyder displays a sunny generosity. Stansky and Winkler actually give us a series of specific book reviews, a method that the former overdoes. Curtis, who really gets going in the nineteenth century and is very interesting on it, seems somewhat out of his depth before that. The most impressive, thoughtful, and thought-provoking contribution comes from Winks, who imparts much order to an enormous range of studies.

The general editor, Richard Schlatter, takes pride in his decision not to impose uniformity on these diverse ways of doing things, and, insofar as the contributors were allowed to express their personalities in the listing, one will agree that the absence of a controlling hand did good. Schlatter was, for instance, right in not ironing out the difference of opinion that describes the same book in one chapter as superficial and in another as informed and eminently readable. The volume would have gained greatly, however, from rather more planning and control. A standardized system of referencing would have helped; there was no need for so much variety in the use of footnotes. The index both displays and condemns editorial self-restraint. Not all its page references are accurate. Thanks to the fact that some contributors mention authors' names in their text as well as their notes, the references give apparent massiveness to some scholars that their achievements do not support: since, in addition, books covering more than one section appear artificially often in the index, counting entries there can give a very false impression as to the productivity and influence of some writers. Because no policy was imposed touching the use of forenames or initials, Ruth Dudley Edwards gets credited with the work of her father (Robert) as well as her own. Indexing should have helped to avoid so blatant a solecism as the three various spellings of Alan Macfarlane's name in the text, the correct one not being that used in the index. Occasionally the index gives up. Thus, Peter Lake, incomprehensibly transmuted in MacCaffrey's contribution into "Cable" (p. 96), does not appear in either guise, though the index duly endorses the curious error in the same piece that substitutes Robert A. Smith (one of the contributors) for Lacey Baldwin of the same name (p. 71).

The unavoidable cutoff date created inescapable problems, but some contributors made things worse by incautious assertions. Thus, Hanawalt, going down to 1980, remarks on the absence of a major biography of Henry VI (p. 52); two vast volumes on king and reign appeared in 1981. Underdown calls Christopher Hill's *Economic Problems of the Church* (1963) "definitive" (p. 117), just ahead of the book's effective demolition by new work. MacCaffrey's just complaint about the paucity of work on Tudor foreign policy is in the process of being met by such scholars as E. I. Kouri and Simon L. Adams.

Such blemishes are in part inevitable in an enterprise of this kind but in part would have been prevented by more stringent editing. Together with some of the contributors' idiosyncrasies, they detract from the usefulness of the book. Nevertheless, in sum, this is a brave and often helpful undertaking. Both the achievements and deficiencies of one of the most productive and lively sectors of the world's historiography often stand out clearly and instructively. It is noticeable that the traditional concerns of British scholarship remain very active, despite the modish desire for social history, which receives starry-eyed endorsement here and there in this volume. Curtis (p. 431) laments the lack of such work in Irish history—women, the family, popular culture, and the rest. In fact, Irish historians, still engaged in clearing much ground overgrown with the nettles and brambles of nationalist traditions, have got their priorities right. Underdown writes like a recent and therefore passionate convert. He builds his piece around the supposed program of social history put forward by Keith Thomas in 1966; yet, what is really dominating his period is the total rewriting of its basic political story in work that, insofar as it appeared in time for this survey, he seems to regard as unfortunate. He only once complains of an omission. Referring to a seventeenth-century minister of religion whose recently published diary has attracted much attention, he comments that "unfortunately, Josselin's reticence deprives us of information on the diarist's sex life and other important matters" (p. 102). Important?

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JUDITH H. ANDERSON. *Biographical Truth: The Representation of Historical Persons in Tudor-Stuart Writing*. New Haven: Yale University Press. 1984. Pp. ix, 243.

Old wisdom insists that truth is stranger than fiction. That might well be the case. It might also be, as Judith H. Anderson argues in her fascinating study of Tudor-Stuart biography, that truth is not estranged from fiction. Her objective in writing *Bio-*

graphical Truth is "to demonstrate how an awareness of truth in fiction, or of fiction in truth, exists with increasing complexity in Renaissance depictions of historical figures" (p. 202). After examining her argument, most readers will undoubtedly agree that Anderson not only reached her objective but also, in the process, laid claim to a prominent place among the ablest of Renaissance scholars.

Anderson draws boundaries between history and biography, maintaining that "history must deal with actual events and facts, whereas life-writing must have in it a considerable element, not of untruth, but of fiction" (p. 79). By this she means that the life-writer may say nothing false and must also write vividly, affectively, and dramatically—calling on the gifts of intuition, imagination, and "feigning." One should note that Anderson defines fiction the same way that Izaak Walton did: "subjective, imaginative truth," which differs from "objective, factual truth" but is nonetheless "true" (p. 52). Biographical truth, then, is not perfect representation; it is an honest interpretation of the subject to the best of one's observations, insights, and intuitions. Renaissance painting offers a helpful analogy—Velazquez standing for the unmediated objective likeness of history and El Greco for the interpretive (psychological, moral) likeness of biography.

The first third of *Biographical Truth*, which treats early life-writing (Venerable Bede's *Life of Cuthbert*, George Cavendish's *Life and Death of Cardinal Wolsey*, William Roper's *Life of More*, and Walton's *Life of Dr. John Donne*) is far from being a mere rehearsal of modern historiographical studies. The chapter on Bede's *Life of Cuthbert* is a commendably original discussion, demonstrating precisely how medieval biography combined historical facts, subjective interpretation, and rhetorical amplification to make of truth and fiction a seamless garment that fits the needs of the age like a liturgical vestment. With a brilliant display of literary analysis, Anderson uncovers in Cavendish's *Life and Death of Cardinal Wolsey* the extraordinary tension caused by his increasingly ambivalent attitude toward his subject. Her discussion of Roper's *Life of More* shows how Roper's ingenious arrangement of material, dramatic analysis of motive, psychological nuances, foreshadowing, and "retrospective shading" constitute an artificial design that paradoxically makes his text a truer representation of Sir Thomas More's humanity and sanctity than Harpsfield's more factual biography.

The second part of *Biographical Truth* concentrates on More's *History of King Richard III* and William Shakespeare's *Richard III* and *Henry VIII*, each of which deals with a historical figure but with a much different consciousness and use of history. Again one is impressed by the strength of Anderson's argument, the frequency of lucid, com-

elling insights, and the subtlety of her critical discriminations. The discussion of *Henry VIII* is especially inventive, leaving the reader quite convinced that Anderson is right to claim that the play is conspicuously attentive to the interaction of truth and illusion, to ambiguity, double-mindedness and self-deception, to the means by which humanity fashions fiction to look like truth, and, ultimately, to the "interpretative process itself" (p. 153).

In the final third of the book, the author looks closely at the nature of truth and what one might call truth-consciousness in Francis Bacon's *Henry VII*. Here the principal concerns are "true history," "equivocal truth," and the historian as "inventor." The cast of historical figures and historians that Anderson maneuvers through the last chapters is formidable indeed: Henry VII, Elizabeth of York, Margaret of Burgundy, Richard III, Edward Lord Herbert, Jasper Tudor, Cardinal Morton, the Duke of Buckingham, Polydore Vergil, Joseph Hall, Bernard André, More, Niccolò Machiavelli, Perkin Warbeck, Lambert Simnell, and others. Her well-braced conclusion that Bacon wrote *Henry VII* to play up similarities between Tudor king and Stuart biographer offers the reader the kind of reasonable provocation that makes scholarship the rich experience it should always be.

Biographical Truth is provocative for other reasons as well. Readers will be quickly and recurrently aware that epistemology is at issue in addition to historical truth, the truth of fiction, and the fiction of truth. How do we know? What do we know? How accurately can reason and language communicate what we perceive to be true? Anderson does not argue that there is no absolute truth but rather that we cannot know truth absolutely, least of all the full truth about other people.

This is a book that rests solidly on an admirable balance of theory and analysis. Neither the argument nor the reader's interest ever thins out, primarily because Anderson fits a very sophisticated argument to indisputably substantial texts. Her achievement is distinguished by singular insights, remarkable organizational skill, and judicious enthusiasm for the subject, which most readers are bound to share.

JOHN X. EVANS
Arizona State University

VINCENT CARRETTA. *The Snarling Muse: Verbal and Visual Political Satire from Pope to Churchill*. Philadelphia: University of Pennsylvania Press. 1983. Pp. xxi, 290.

Librarians contemplating the purchase of this book will want to know that the "Churchill" in its subtitle

is not Winston but Charles, a minor poet who died in 1764, only twenty years after Alexander Pope. In other words this is very much a book for specialists of the eighteenth century. In origin a dissertation on Pope, the book discusses the relationship between satirical poetry—mainly Pope's—in the age of Robert Walpole and the emblematic engravings published at that time. Vincent Carretta argues that conditions just then were especially favorable to satire. In particular, Walpole's long ministry gave his opponents a unique opportunity to develop a consistent and effective rhetorical strategy; when political instability followed his fall, personal abuse displaced generalized poetical satire and emblematic prints gave way to the caricature of individuals.

Carretta has read widely, and the epilogue and some of his specific observations—for example, on Pope's *Imitations of Horace*—are interesting. But too much of the book is discursive and repetitive, and he uses evidence and argument in ways that will weaken the confidence of many readers in his assertions about both politics and poetry. Elizabeth I should not be called a "limited monarch" (p. 2). There was no earl of Temple (p. 243) nor has St. Paul's a "steeple" (p. 28). Non-Namierite historians do not "spend a great deal of time describing the positions of the Opposition [to Walpole] and arguing that the ideology found in Bolingbroke's writings was sincere" (p. 33). It is improper to apply to the 1730s a partial quotation from Geoffrey Holmes, which in fact refers to the years 1702–14 (p. 33.). Few historians today would refer flatly to Walpole's "corrupt system" (p. 159). And as an example of naivete there is this remark: "As historians have increasingly come to see, 1760 marks a major turning-point in British political history" (p. 260). (That would make a good examination question in a course on historiography!)

Carretta's overriding concern with his political thesis leads him at times to misread Pope and to trivialize this major poet into a mere "test case" (p. xix). In a sense Pope did owe much to Walpole's regime for providing a subject ideally matched to his satirical talents. But he owed even more to the tradition and manner inherited from past writers—especially John Dryden—which he polished to perfection. Carretta ignores this; here all is politics. Thus, the great opening of *Dunciad*, II, which describes Colley Cibber enthroned, is taken as referring to Walpole's elevation to the peerage; there is no mention of its derivation from Milton, which is the real point. The brilliant portrait of Lord Hervey as *Sporus* reflects, we are told, not intense personal loathing on Pope's part but a disagreement with Hervey's "policies" (p. 115). This is a monumentally wrong-headed judgment.

I should add that Carretta on the whole writes well and that his publisher should be thanked for

the number of political prints included, although some of them are too small and too murky to be studied in detail.

THOMAS W. PERRY
Boston College

RICHARD BAUMAN. *Let Your Words Be Few: Symbolism of Speaking and Silence among Seventeenth-Century Quakers*. (Cambridge Studies in Oral and Literate Culture, number 8.) New York: Cambridge University Press. 1983. Pp. viii, 168. Cloth \$32.50, paper \$9.95.

In *For the Reputation of Truth* (1971), Richard Bauman of the University of Texas examined the Quakers' concern for the Seneca Indians in terms of the Quakers' complex social and political roles during the French and Indian War in Pennsylvania. In *Let Your Words Be Few*, he has concentrated on an earlier period and probed the early Quakers' relation to God, their community, and the wider society of Puritan and Restoration England. Bauman draws widely on terms and sidelights from modern social science, but the heart of his presentation is the early Quaker world view of the conquest of humankind by God's spirit. This, since my *Quakers in Puritan England* (1964), is often given James Nayler's apocalyptic title "the Lamb's War." It is presented here more fully than in any other modern work and is rightly linked to traditional Quaker patterns of speech and silence: human wills and voices must be silent before God who can speak to and through them; human conventions of speech in both worship and courteous society are the work of human wills and Antichrist. The deliberate discourtesies of the early Friends expressed "true" grammar and human equality and were deliberate assaults on the pride built into human hierarchies. Oaths put the commands of the ruler above those of God in the Sermon on the Mount (also, the writer may note, they enlist God's judgment for human purposes). In silent Quaker worship, tensions between the two calls—to avoid speaking out of self-will and to avoid "quenching" the "openings" of the Spirit—led first to the "quaking" and later to the "eldering" of those who spoke. In his eighth chapter Bauman senses the religious as well as the social dimensions of Quaker messages—"leadings" and concern for truth, for instance—when an early Friend had to discern whether sudden personal insights were meant for him- or herself, for building up the meetings, or for non-Quakers.

Bauman's solid array of quotations and citations includes not only many modern studies and unpublished theses on the social functions of speech, dramatic sign-acts, and show trials but also a wide-

sampling of early Quaker writings, though many are from Victorian editions by way of secondary collections such as *Letters of Early Friends* and *Early Quaker Writings*. The text is well set and free of errors.

Bauman presents, with fairness and depth and without moral or theological judgment, the conviction of Quakers that God led and spoke to them constantly and the distinction drawn by early Quakers between self-will and the voice of God. The social science least used is psychology, although Bauman does write of a Friend who, having read of prophetic gestures by fellow Quakers and the prophet Isaiah, suddenly felt led to "go naked as a sign" to his spiritually naked townsfolk, but knew he had to resist his own selfish shame along with his neighbors' scorn. Unlike the Ranters, the Quaker often tested his motives against the Bible and other Friends' consciences, but he did not know to ask a Freudian's further questions. For early Friends, salvation was gained through suffering because it was a purging of self-will (they shrewdly warned, however, against self-imposed self-sacrifice), but they did not ask whether aggression against others or against impulses within themselves underlay their confrontations of others' consciences. We, who for our own sakes must ask such questions, no more need to judge early Friends than does Bauman.

HUGH BARBOUR
Earlham College

F. J. MCLYNN. *The Jacobite Army in England, 1745: The Final Campaign*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1983. Pp. ix, 210. \$28.50.

When, on December 5, 1745, the Jacobites made the fateful decision to retreat from Derby, a contemporary observed that "even what has happened already must appear to posterity liker a Romance than any thing of truth" (p. 123). If posterity has, in general, concurred, there is little of this romance in F. J. McLynn's study of a topic all too often overly romanticized. He laboriously details the last military campaign on English soil, literally providing a day-by-day account from November 8, when Prince Charles and his army first crossed the Scottish border, until December 20, when some 2,000 Highlanders braved the neck-deep torrent of the River Esk in order to get back to Scottish soil.

The entire episode was on both sides a remarkable exercise in futility. The Jacobite command was characteristically riven with faction, while the Hanoverian response to the invasion was largely inept. In describing the advance through Carlisle, Lancaster, Preston, and Manchester to Derby,

McLynn convincingly demonstrates how surprisingly well disciplined the Jacobite forces were. The civilian population was treated with courtesy and respect, and propagandist Whig atrocity stories were almost totally without foundation. The retreat was another matter. Disenchanted and demoralized, the Gaels vented their wrath and frustration on the hapless population. Nonetheless, the Jacobites made a more impressive showing than the troops commanded by "grandmother Wade" as the Duke of Cumberland called him. Indeed, Wade's inactivity and incompetence largely explains the success of Charles's retreat and the extension of the campaign by another four months until it finally ended in the carnage of Culloden.

Two chapters in this study particularly stand out: one on the composition of the Jacobite army, which indicates the complexity of clanship and allegiance in the Scottish Highlands, and another on the "Decision at Derby." The latter is an insightful and illuminating discussion of a highly familiar topic. The author's complementary studies of the French role in the Forty-Five lead him to favor the arguments of Charles Edward—the army should have advanced on London. Lord George Murray, who is otherwise recognized as the military mastermind behind the campaign, was wrong to counsel retreat. The case is not closed. It could be objected that Jacobite intelligence was so inadequate that no one, least of all Charles, who, like so many leaders of the Stewart cause, fought ignorance with wrong-headed optimism, could be sure of French support when most needed. Communications with English Jacobites, who were supposed to rise as the invaders advanced, were pathetic, and, indeed, English inaction rendered retreat inevitable. A majority of the chiefs sided with Murray and most understood with Sir Thomas Sheridan the significance of the decision: "It is all over, we shall never come back again" (p. 129).

At times, the author overburdens the text with information to the point of antiquarianism. It is not essential to know that David Morgan rode a bay horse (p. 138) or that on December 12 the Duke of Kingston "lodged at the house of Mr. Houghton at Baguley Hall near Northenden" (p. 157). To compare the invasion with Mao's "Long March" or Xenophon's account of the withdrawal of the 10,000 (p. 197) seems fanciful, but McLynn's undoubted achievement is to demonstrate that the campaign was "an outstanding military exploit." His thoroughness and industry alike ensure that this subject is now closed. This will be the last book on the last campaign on English soil for some considerable time to come.

EDWARD J. COWAN
University of Guelph

EARL KENT BROWN. *Women of Mr. Wesley's Methodism*. (Studies in Women and Religion, number 11.) New York: Edwin Mellen. 1983. Pp. xvii, 261.

Earl Kent Brown believes, with good reason, that historians have unduly neglected the Methodist women of the eighteenth century; he intends to right the balance. His book consists of two long sections, entitled "Leadership Roles of Women" and "Women of the Word" respectively, sandwiched between an introductory statement regarding Wesleyan Methodism and a "group portrait" of the 108 women whom he studied. An appendix briefly identifying each of these individuals, a bibliography of works cited, and an index round out the volume.

In his first section Brown pays particular attention to women as "speakers of the Word," itinerants, and school leaders, while also noting their activities as advisers and counselors, band and class leaders, visitors, ministers' wives, members of "support groups," patrons, and models of the Christian life. He notes that Wesley, after overcoming his initial hesitation, accepted women as "preachers." Brown also notes the contrast between Wesley's judgment in this matter and that of Victorian Methodists. The biographies of six prominent women are to be found in the second section and include Lady Darcy Maxwell; Mary Bosanquet Fletcher; Elizabeth Ritchie Mortimer; Sarah Crosby; Selina, Countess of Huntingdon; and Hester Ann Roe Rogers. These biographies are basically descriptive in nature, with relatively little by way of analytical or interpretative comment.

This book brings together in one place information not readily available to the public; for this, the author deserves our thanks. The data have been summarized in a manner that will make them easily accessible to students and others interested in Methodist history. The even tone that Brown adopts, however, eventually goes flat. This contrasts with the sprightly character of another recent book that covers some of the same ground, Maldwyn Edwards's *Dear Sister: The Story of John Wesley and the Women in His Life*.

Brown is careful not to go beyond the evidence; he points out that his conclusions are based on a limited number of cases and that his sources provide partial information at best. Although recognizing the problems that anyone researching this subject must confront, one may ask if Brown has collected as much data as he might have. I found no references to Martha Thompson Whitehead and Nancy (Ann) Bolton, who are at least as well known as some of those whom he mentions. As regards manuscript material, Brown seems to have depended primarily on the Methodist Research Centre in Manchester. Might he have found some useful material in private hands or in other depositories had he cast his

net wider? For instance, the collection of Lady Huntingdon's correspondence held by the Chesham Hunt College Foundation, Cambridge, might have provided helpful insights that he could have used in his biography of the countess.

JOHN C. ENGLISH
Baker University

ROBERT GLEN. *Urban Workers in the Early Industrial Revolution*. (Croom Helm Studies in Society and History.) New York: St. Martin's or Croom Helm, London. 1984. Pp. 348. \$30.00.

This study of the Stockport district in the period 1780–1820 is a thorough piece of research based on extensive examination of printed and manuscript materials. Chapters on social, economic, and political structures are followed by narrative accounts of radical and protest movements ranging from Jacobinism and Luddism to the March of the Blanketeers and the demonstrations of the Peterloo era. Although the book stops short of Chartism, there is a chapter on the fragmentation of workers' movements in the 1820s. Eclectic and pluralistic in approach, Robert Glen's work sticks closely to the sources and refrains from imposing an external pattern on the facts. The book will be of considerable interest to specialists. Its usefulness is enhanced by an excellent index.

The author takes pains to establish the relationship between his findings and received interpretations. In an introductory chapter, "Debate over the 'Working Class,'" he explores the theoretical and historiographical thickets that have burgeoned during the past quarter century and defines his own study as, in part, an attempt to "test the hypotheses" of John Foster, Harold Perkin, and E. P. Thompson. He does this, in part, in the course of the narrative; the chapter on Luddism, for example, describes a number of weaknesses in Thompson's interpretation. In his last chapter Glen presents three rather negative overall conclusions: both workers' solidarity and their "conflict orientation" were severely limited, and "the solidarity of the middle and upper classes and their hostility towards workers has been vastly over-rated" (pp. 278–79). Throughout the book Glen emphasizes the diversity characteristic of Stockport workingmen and rejects the notion that there was a "working class."

Although Glen has approached the problem of interpretation in a serious and careful way, he has run into difficulties. In defining his interpretive project primarily in terms of the evidentiary relationship between local data and the "national conclusions" put forth by his predecessors, he has been deflected from the task of providing a sustained analysis of developments in Stockport, which tend to

be treated on a rather factual level. Moreover, since the three historians under scrutiny operate within very different conceptual frameworks, the isolation of discrete hypotheses for testing is a somewhat factitious exercise, which bypasses a range of questions concerning the fit between the constructs and the phenomena they purport to describe and explain. The assimilation of Thompson's magisterial study cannot be accomplished by breaking it up into component conclusions in the mode sanctioned by the historiographical discourse of the late nineteenth century. Similarly, the analytical function of local history should not be limited to the empirical validation or refutation of established interpretations and theories. Of course, local history cannot stand by itself; it must be set in a larger framework. One of the merits of Glen's book is that he has confronted the difficult problem of defining the relationship between his contribution and a body of knowledge whose conceptual foundations are shifting.

TRYGVE R. THOLFSEN
Columbia University

P. W. J. BARTRIIP and S. B. BURMAN. *The Wounded Soldiers of Industry: Industrial Compensation Policy, 1833–1897*. (Oxford Socio-Legal Studies.) New York: Clarendon Press of Oxford University Press. 1983. Pp. xi, 253. \$29.95.

Before 1833 the English government did nothing to reduce industrial accidents and, except for a rare case in common law, nothing to see that a victim was compensated. By 1897 a multitude of laws, bureaucrats, regulations, courts, and court decisions involved the government in the prevention of accidents and the compensation of its victims. P. W. J. Bartrip and S. B. Burman describe these complicated developments in *The Wounded Soldier of Industry* and do so with a probing, skeptical, tough-minded shrewdness.

They are skeptical, for example, that government intervention reduced accidents. Where such intervention was slight, as with railways, accidents declined as much as in mining where intervention was great. Technological advances, better job training, and a greater concern for life, not government, made industries safer. The authors are also skeptical that the famous Workmen's Compensation Act of 1897—with its employer liability, employer-employee contributions, and no-fault insurance—was the inevitable result of industrialization or bureaucratic growth. The 1897 act, for example, owed nothing to bureaucrats. It was also opposed by both trade unionists, the vanguard of the proletariat, and the politicians and businessmen of the propertied classes. The 1897 act, argue Bartrip and Burman,

was a compromise—one liked by neither party but, given the impasse created by conflicting laws, court decisions, and interests, a necessity. It was also an idea devised by lawyers and arising from earlier thinkers.

Although Bartrip and Burman are convincing when they state that the particular formula of the 1897 act was not an inevitable result of industrialization, they are less so when they deny that a "collectivist solution was . . . adopted out of necessity" and deny that it "was not dependent on any individual" (p. 219). That some kind of solution was, in fact, necessary and was not dependent on any individual actually underlines the strongest parts of their analysis. Although they disavow the sovereignty of impersonal forces, their most cogent arguments deal with economic and class forces—with trade cycles, railway men in Parliament, and trade unionists at the polls. Their implicit economic interpretation even becomes explicit when they write of the "widespread interest" in efficient and just laws as "resulting from the Industrial Revolution" (p. 98).

Bartrip's and Burman's description of "how" the government became involved in safety and compensation of industrial labor is far more powerful than is their theory of why this happened, a theory to which they devote only half a page. But, since all descriptions of "how" do implicitly involve a "why," their always clear, correct, and perceptive descriptions offer a most illuminating and reliable account of an important event in the history of Victorian England.

DAVID ROBERTS
Dartmouth College

JAMES A. SCHMIECHEN. *Sweated Industries and Sweated Labor: The London Clothing Trades, 1860–1914*. (The Working Class in European History.) Urbana: University of Illinois Press. 1984. Pp. 209. \$23.95.

James A. Schmiechen's book, *Sweated Industries and Sweated Labor: The London Clothing Trades, 1860–1914*, draws attention to "sweating" as an aspect of the centrifugal as opposed to the better-documented centralizing forces of industrial production at this time. He points out the growth of a symbiosis between factory and nonfactory production and includes an examination of women and Jewish immigrants as important elements in the nonfactory labor force. In the second half of the book he deals with the role of the trade unions and the state in efforts to end sweating and women's part in this struggle.

Schmiechen shows very clearly how the Factory and Workshops acts and the difficulties of a small inspectorate in supervising a vastly greater number

of workplaces encouraged home and outwork (pp. 53, 135 and following). He gives interesting analyses of several aspects of the growth and ultimate checking of sweating: the attitudes of employers to outworkers and homeworkers (pp. 52–59); the impact of machines in clothing and footwear (chap. 2); and the pressure brought to bear by upper- and middle-class radicals and some trade unionists to end the system, culminating in the passage of the 1909 Trade Boards Act (pp. 91–2, 61 and following, 134). Schmiechen also provides some very useful tables and interesting footnotes. He is deeply aware that the true history of the British working-class and labor movement cannot be written unless women are shown as integral to these developments.

When Schmiechen strays from the London experience and his period, however, the book develops some serious shortcomings. He appears to accept the Women's Trade Union League's own estimate of its importance and that working women "remained generally unorganized" not only in London but "throughout Great Britain" (p. 93). He accepts the middle-class overemphasis on the Match Girls' Strike of 1888 (pp. 87, 115–16, n. 21). In fact, union and friendly society rule books, local newspapers, and *The Pioneer* (voice of the Grand National Consolidated Trades Union) attest to the fact that organization of women began at the same time as that of men and that, although on a smaller scale, women made a contribution to the first phase of British trade unionism. Newspapers and journals show the continuing efforts of women to organize. Textile and, later, white-collar unions were dependent on women's membership and, in some cases, leadership and initiative. Schmiechen is hazy about the history of sweating (pp. 2–3); the term may have originated in the 1840s, but the practice is noted in the records of small sixteenth-century towns when the authorities were allocating alms to seamstresses or "sewsters" as well as "poor spinners" because of the growth of a landless, urban population. The middle class did not suddenly sprout a conscience about the poor in the late nineteenth century (p. 134); they developed it in Elizabethan England and much more strongly during the mid-eighteenth century when it was manifested in Wesleyanism, welfare work, and radicalism. He recognizes "the displacement of artisan husbands in traditional crafts" by women (p. 67) but has little to say on the displacement of women by men. Wives thankfully left paid work partly because their emulation of middle-class living standards increased their unpaid domestic work; it was no "release from work." The book needs to be seen in wider context, but within its limits it is a notable contribution to the understanding of British labor hierarchy.

SHEILA LEWENHAK
University of London

TERRY M. PARSSINEN. *Secret Passions, Secret Remedies: Narcotic Drugs in British Society, 1820–1930*. Philadelphia: Institute for the Study of Human Issues. 1983. Pp. xii, 243. \$17.50.

Those of you who, like the present reviewer, are not professional historians but derive pleasure and benefit from reading history must always be grateful to the scholar who writes a book that has the stamp of authority and yet is comprehensible to laity. I am grateful to Terry M. Parssinen but wish he had not thought it necessary to set a bait with an absurdly inappropriate title and a jacket illustration of a girl injecting herself, which is taken from an American movie.

My own understanding of the history of drug use in Britain has been specially informed by the work of Virginia Berridge. From 1976 onward she published a series of highly original contributions on this topic. In 1983 along came Parssinen's *Secret Passions, Secret Remedies*, which explores the same territory previously opened up by Berridge. His work must, therefore, inevitably be judged in terms of whether, to any worthwhile extent, he has been able to take things further than Berridge.

In many instances two scholars have moved into the same field at more or less the same time, and such competition can be fruitful if it succeeds in uncovering different sources and developing different overall interpretations and perspectives. To some extent there has certainly been such a happy outcome in this case. For instance, Parssinen's chapter on "Illicit Drug Use in the Twenties" stands as an important essay in its own right, breaking new ground. He uncovers information on court sentencing at this time, which is highly informative. At various points from De Quincey onward, he shows a sensitive ability to deploy popular literature and newspaper reports; his short chapter on "Agents of Corruption" skillfully uses such sources to describe developments in popular perceptions of the drug user from 1910 onward. An interpretative chapter, "Narcotic Drugs in Britain and America," is admirably balanced, warning against the assumption that the low number of drug problems in Britain between the two world wars was a consequence of benign policies; British policy makers could afford to take a relatively relaxed view of the problem simply because it was miniscule.

For these and other reasons, there can be no doubt that anyone interested in the story of one particular country's sometimes shifting, sometimes benign, sometimes posturing, and often muddled responses to drug problems should read both Berridge and Parssinen; they are, in many respects, happily complementary. In other chapters, however, Parssinen has walked in Berridge's shadow; his chapters on the international opium trade, popular

use of opium in the nineteenth century, the developing antiopium movement, the genesis of legal controls, and the 1926 Rolleston Committee will be unexciting to anyone familiar with Berridge's work. And, at the end of the day, Parssinen does not succeed in offering analytical insights into the nature of the larger social processes shaping drug policies that would take us significantly beyond Berridge's position.

GRIFFITH EDWARDS
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London, England

FRED KAPLAN. *Thomas Carlyle: A Biography*. Ithaca: Cornell University Press. 1983. Pp. 614. \$35.00.

This carefully crafted and intimate study of Thomas Carlyle easily wins acclaim as a model of the biographer's art. Fred Kaplan's considerable talent gives readers entrance to the very rooms occupied by Carlyle; we are seemingly present at private conversations and privy to Carlyle's mental processes. Kaplan demonstrates his own exasperation with this man, which also must have been intensely experienced by those closest to him, in particular by his remarkable spouse, Jane Welsh. From his birth and early years in Ecclefechan through Carlyle's last major literary effort, the *History of Frederick II*, Kaplan portrays genius struggling with itself. Driven by his passion to achieve greatness as a writer, Carlyle fell prey to early fears that he would measure up to be less than he could be. In spite of agonizing over making his living and fearing that his physical ailments would lead to an early demise, Carlyle did achieve greatness. When success finally arrived in his mature years, he was scarcely able to accept graciously reverence as a Victorian sage.

Carlyle was not himself a heroic figure. His egomania was crippling, and he cannot be forgiven for the severe emotional damage he inflicted on his wife. Yet Kaplan is not ungenerous with his subject, for he makes the reader understand much of Carlyle's social and intellectual heritage and his inner compulsions without resorting, thankfully, to psychological formulas. To have been Carlyle, and to have lived in an age in which the many webs of modernization and secularization were emerging to smother individual genius, was a great accomplishment. *Sartor Resartus*, *Heroes and Hero Worship*, *The French Revolution*, *Cromwell*, the political *Latter-Day Pamphlets*, and *Frederick* were Carlyle's means to create a self. Unable verbally to express ideas, his books, not people, became the objects of his devotion.

Kaplan neither provides much by way of critical reassessment of Carlyle's major works nor does he react to the voluminous secondary materials but

confines this study to the details of Carlyle's personal and literary life. Relying chiefly on correspondence to reveal the character of his subject, Kaplan's study falls into the greatest tradition of Western biography. His writing is brilliant, although the strict obedience to chronology does, at times, tend to obscure the more important episodes of Carlyle's life. Some readers may contend that Kaplan dwells too much on the more minor marital conflicts and the many real or imagined physical ailments of both Thomas and Jane.

It may be true that the writings of Carlyle offer little satisfaction for the twentieth century; this biography does not compel a desire to reread them. The real contribution of this beautifully prepared book is that, by revealing the workings of a Victorian genius so sensitively, Kaplan does contribute significantly to the elevation of our understanding of the Victorian mind. This work is the definitive biography of Thomas Carlyle. For decades to come it will not find serious challenge to that status presented by any competing volume.

LEE E. GRUGEL
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Eau Claire

STEFAN COLLINI *et al.* *That Noble Science of Politics: A Study in Nineteenth-Century Intellectual History*. New York: Cambridge University Press. 1983. Pp. x, 385. Cloth \$49.50, paper \$14.95.

This brilliant study by three remarkably gifted and accomplished intellectual historians assesses some of the diverse attempts of various nineteenth-century British thinkers to construct a conceptual foundation and methodology for a "science of politics." Distinguishing all of these enterprises was an earnest conviction that a systematic analytical understanding of man's civic life was indispensable to his practical improvement.

Stefan Collini, Donald Winch, and John Burrow disavow at the outset any notion that their book is a contribution to the history of political science as a discipline: "The teleological history of disciplines superimposes the intellectual map of the present, or some version of it, on the usually significantly differing ones employed in earlier periods, often to the point of obliterating them entirely" (p. 4). As intellectual historians they are rightly concerned with the role writers on politics saw for themselves in relation to their world, and they consequently stress the need to explore the social and cultural terrain that circumscribed their discourse and conferred explanatory power on it. This commitment informs the authors' consideration of Dugald Stewart and his influence on the Edinburgh reviewers, the intricate intellectual engagement fought by Whig reformers

and Benthamite radicals, the tension inherent in J. S. Mill's ambitious endeavor to place the study of politics on a new footing through the application of a methodological synthesis, Walter Bagehot's subtle yet robust dissection of the pieties of mid-Victorian political life, the rise and impact of the comparative method associated with Henry Maine, E. A. Freeman, and John Seeley, and the late nineteenth- and early twentieth-century responses of Henry Sidgwick and Alfred Marshall to the limitations and achievements of their predecessors.

That Noble Science of Politics is a book of recurrent themes. One is the debate over the relative weight to be given to induction from historical experience and deduction from the fundamental elements of human nature in the effort to acquire essential political knowledge. A second is the massive and persistent authority of political economy—whose abstract properties enhanced rather than diminished its jurisdiction over questions of great practical import—in the definition and discussion of political issues. A third is the notable degree of continuity throughout the century in both the categories used to examine the political life of society and in the institutional and cultural structure that shaped the examiners.

It is perhaps invidious to introduce distinctions among the authors, each of whom invariably displays a command of his material and a powerful, lucid, analytical intelligence. Yet it is evident from the preface that Collini was the driving force behind this volume, and he imparts a luminous energy to it. In those essays that clearly bear his mark, a fluidity and swiftness of movement combine with a stinging punch (which in the case of J. S. Mill may occasionally land a trifle below the belt) to make for an exhilarating and compelling performance.

BRUCE L. KINZER
University of North Carolina,
Wilmington

NICOLAAS A. RUPKE. *The Great Chain of History: William Buckland and the English School of Geology, 1814–1849*. New York: Clarendon Press of Oxford University Press. 1983. Pp. xii, 322. \$45.00.

Until recently, most histories of nineteenth-century British geology have been characterized, and to some extent distorted, by a post-Darwinian perspective. Most have also been influenced by Charles Lyell's criticism of his predecessors and contemporaries in the "historical" chapters of his *Principles of Geology*. In particular, Lyell's and Charles Darwin's clerical-naturalist predecessors have been portrayed as blinded by theological prejudice, especially by their attachment to the Noachian deluge and the first two chapters of Genesis. The flaws in this interpretation have become obvious, and in the past

decade a number of reassessments have appeared. Nicolaas A. Rupke's well-written monograph is a welcome addition to this revisionist literature.

William Buckland, the first reader in geology at Oxford, canon of Christ Church, and dean of Westminster, was one of the most famous scientific figures of his day. He was an indefatigable field geologist, an advocate of controversial new geological theories, an influential teacher, and an enormously popular public lecturer. Buckland was also a conscientious churchman and a self-conscious eccentric. Renowned for his knowledge and ability, he presented himself to the public and his peers as a learned buffoon, and thus has he usually been presented by historians. As portrayed by Rupke, Buckland had no problem reconciling his Broad Church theology with the growing geological evidence for vast pre-Adamic time spans, progressive change of living forms over time, and even the relegation of the Noachian deluge to a relatively minor event in the geologically recent past. His problem was to make these views, and the whole subject of geology, acceptable within the singular intellectual, cultural, and religious context of Oxford's educational system. Rupke argues that this task led Buckland to establish a uniquely English "school" of geology.

Rupke delivers both more and less than is promised in his subtitle. He has written a provocative reinterpretation of a key episode in the history of English geology and yet avoids the contentiousness that mars so much revisionist scholarship. This is not another chapter in the history of the conflict between science and religion. Buckland and his fellow clerical-naturalists are presented not as defenders of orthodoxy but as champions of a progressive new science, seeking to make their science acceptable within the traditional structure of the ancient universities. The scope of Rupke's research is impressive. He effectively blends social and cultural history with the history of ideas. He sorts through the confusion created by the uncritical use of anachronistic labels, such as neptunism, vulcanism, catastrophism, and uniformitarianism, and provides a perceptive analysis of the social and intellectual context of Buckland's geology.

Rupke's defense of Buckland is secondary, however, to his central thesis: an ambitious attempt to define a uniquely English (as opposed to *British*) school of geology. It is here that the limitations of his argument are most apparent. He makes a convincing case for an Oxford school and, perhaps, even for a more general English preoccupation with historical geology, but his perspective is ultimately too narrow to encompass his broader aim. His focus on Oxford has led him to elevate Buckland at the expense of his contemporaries, to neglect the very real differences between the educational traditions

at Oxford and Cambridge, to denigrate the work of Adam Sedgwick (Buckland's influential contemporary in the Woodwardian chair at Cambridge), and to ignore almost entirely those London geologists who made the Geological Society of London a center of scientific activity and debate. If there was an English school of geology rather than merely an Oxford school, these other branches of the school will require a more balanced assessment.

JOE D. BURCHFIELD
Northern Illinois University

ADRIAN DESMOND. *Archetypes and Ancestors: Palaeontology in Victorian London, 1850–1875*. Chicago: University of Chicago Press. 1982. Pp. 287. \$22.50.

The social construction of scientific knowledge, clearly one of the most exciting trends in the history of science in the 1980s, has made a solid stride forward with the publication of *Archetypes and Ancestors*. Taking his cue from the Edinburgh school (Barry Barnes, Steven Shapin, and others), Adrian Desmond set out to determine how much light might be shed on the mid-Victorian controversies over fossil reconstruction by an investigation of the ideological commitments and political programs of London paleontologists. The answer is: a great deal of light. The resulting book is thoroughly fascinating.

The rise of vertebrate paleontology occurred at precisely the same moment as the great pressures for the professionalization of British science, and Desmond shows how intimately these two movements were connected. He draws numerous personalities into his narrative, including some, such as Robert Edmond Grant, William Kitchen Parker, John Whittaker Hulke, and Harry Seeley, who are well deserving of fresh treatment. But it is the idealist Richard Owen, sculptor of the "archetype," and the positivist T. H. Huxley, promoter of "ancestries," who are the bright spots, and the extremes, of Desmond's spectrum. He resuscitates the long-maligned Owen to a position of credibility, if not sympathy, while tarnishing the heroic Huxley by exposing his merciless, bourgeois, science-reforming instincts. Special attention is accorded to Huxley's curious concept of the "persistence" of fossil groups, a notion he clung to long after the appearance of Darwin's *Origin of Species*, perhaps because he invented it more for political than for scientific purposes.

Some of Desmond's episodes are more suggestive than convincing. He likes to cite social and political events that are concurrent with his fossil controversies, but he does not always indicate clearly what relationship is intended, as when he has Owen attempting "to romanticize British biology against a

backdrop of Chartist agitation" (p. 25). And readers accustomed to smooth chronological continuity may be disturbed by the liberty with which he moves back and forth within his slice of the Victorian era. Rather than a temporal sequence, his chapters elaborate patches of a large mosaic whose fine structure consists of overt paleontological statements mixed with covert political aims.

But Desmond softens any anxieties with a wonderfully engaging style that sacrifices none of the requisite scholarly apparatus. Even the most internalist historians of natural history will now find it difficult to view Victorian science in isolation from the sociopolitical dimension, especially any science in which Huxley was involved.

PHILIP F. REHBOCK
University of Hawaii

CLYDE F. CREWS. *English Catholic Modernism: Maude Petre's Way of Faith*. Notre Dame: University of Notre Dame Press or Burns and Oates, Tunbridge Wells, England. 1984. Pp. xii, 156. \$16.95.

This relatively brief book is a very good study of Maude Petre, a close friend of the Catholic modernists George Tyrrell and Friedrich von Hügel. It is set in England during the early twentieth century. A major English Catholic modernist in her own right, Petre now has a biographer. The subtitle of the book describes its contents better than the title; the work is primarily concerned with her life and thought, which it relates to English Catholic modernism. The broader subject of English Catholic modernism, however, is beyond the scope of this book. Von Hügel, who would be a central figure in any extensive consideration of that subject, appears on only eight pages, and other figures (such as Edmund Bishop, Robert Dell, Emile Joseph Dillon, and William Gibson) who would merit some consideration are not mentioned at all.

In the introduction, Clyde F. Crews helpfully links Petre, a descendant of an aristocratic old English Catholic family, with the cisalpine tradition that existed within English Catholicism before the ascendancy of ultramontanist in the latter half of the nineteenth century. His discussion in an early chapter of Tyrrell's influence on her is also helpful. From the years of the modernist controversies to the end of her life, she persisted in considering herself faithful both to Catholicism and modernism. In addition to her analyses of modernism after the controversies, Petre wrote on topics such as the church, the spiritual needs of humanity, the sufferings of war, and the need for wider international understanding.

Crews's view of Petre's works is well balanced, appreciative, but not uncritical. Of the modernist

controversies, for example, he writes: "Just as subsequent history would show the papal-curial approach to have been overly tight, so would it show many of the Modernists to have been somewhat uncritical in their zeal for the modern spirit" (p. 25). He also does not fail to note that Petre, who became Tyrrell's literary executor, destroyed some of his letters and papers.

Drawing on John Heaney's work, Crews usefully singles out three of the major issues—besides biblical criticism—at stake in the modernist controversies: (1) a totally symbolical, nonobjective approach to dogma, (2) exclusive immanence of the divine and revelation (an immanence that excludes transcendence), and (3) total freedom of scientific research from church dogma. Furthermore, he points out that it "would be a separate judgment in the case of each Modernist whether and to what extent his or her own theology crossed critical lines" (p. 27) and that the location of these lines might be understood differently today than in 1907.

I suspect that, in the fields of history and theology, most of the major work on English Catholic modernists has now been done, although Tyrrell's theological views after 1903 could use additional analysis. To conclude, applications of Jungian psychology might further elucidate the personalities and thought of Tyrrell, Petre, and von Hügel since Carl Jung had in common with them an abiding interest in religion and the spiritual life.

WILLIAM J. SCHOENL
Michigan State University

RAYMOND A. JONES. *The British Diplomatic Service, 1815–1914*. Waterloo, Canada: Wilfrid Laurier University Press; distributed by Humanities, Atlantic Highlands, N.J. 1983. Pp. xiii, 258. \$17.00.

This is the first book-length study of the nineteenth-century British diplomatic service, a topic with rather diffuse sources. It can hardly be termed definitive, but it has opened up the field for further work and refuted or seriously called into question a number of persistent myths.

At the beginning of the nineteenth century the diplomatic service was still far from being "professionalized." Sensitive or plum positions at the top were subject to political discretion; recruitment at the bottom and, to a large extent, even promotion in the middle were the result of political or social patronage. By the middle of the century, however, both appointments and promotion had largely been professionalized, in part by the weakening of party cohesion, more by the elimination of the "family embassy" and the increased bureaucratic control exercised by the Foreign Office over the system of attachés, paid or unpaid.

This process was followed and extended by the adoption in the service of recruitment by examination. Although the attachés were still largely engaged in mechanical copying, their work was considered too sensitive for any other than gentlemen, and so their social exclusiveness, as well as an important element of patronage, was preserved by maintaining the system of nomination and the requirement of an independent income. Yet, contrary to common belief, the intellectual capacity of the recruits was not inferior, nor their social origins superior, to those of the Home civil service.

By digging deeply for detailed information about appointments, Raymond A. Jones is able to construct tables indicating clearly that throughout the period under review the social composition of the diplomatic service, along with the civil service in general, merely reflected that of the political elite. "As the composition of that élite changed and broadened . . . so did that of the diplomatic service" (p. 217). Where, as in this case, the point to be made is one of comparison, most of the statistical problems probably balance out. But, for other purposes, the total numbers involved are really rather small and may not justify some of the points so positively made. Thus, the early resignation of some 22 percent of the recruits of 1870–79 and the sacrifice of the £400 a year their families had invested do not necessarily reflect a deep dissatisfaction with promotion prospects. The author should have asked how many of that small sample of eight resigned on marriage or inheritance. After all, the possession of an independent income made such resignations easier, not more difficult. Jones makes a much better point, without the aid of tables, when he observes that the intellectual caliber of the attachés was rising faster than the level of their work.

Neither the structure of the book nor its author's style make for lucid exposition or easy reading. But Jones has collected from a wide range of sources a wealth of personal detail as well as statistics (though what is the evidence for the categorical statement that Sir William White was Adam Czartoryski's son?), and there are as many good stories as tables.

KENNETH BOURNE
London School of Economics

THOMAS G. FERGUSON. *British Military Intelligence, 1870–1914: The Development of a Modern Intelligence Organization.* (Foreign Intelligence Book Series.) Frederick, Md.: University Publications of America. 1984. Pp. xxii, 280. \$25.00.

The subject is exciting and the work's credentials impressive. Thomas G. Fergusson is a career U.S. army intelligence officer; the book is the outgrowth of work done under Theodore Ropp and Richard

Preston of Duke University. The sources include intelligence reports deposited at the Ministry of Defence Library in Whitehall and papers of the Directorate of Military Operations and Intelligence at the Public Record Office. As always with intelligence sources, one must wonder what has been left out. Fergusson builds on the scattered earlier work on the subject, notably *The Intelligencers* by B. A. H. Parritt's (now director of Britain's Intelligence Corps), but goes well beyond it. He provides maps, charts, and photographs of mustachioed senior officers and their working habitats.

Fergusson's themes—organizational development, national security implications, and tactical or "field" intelligence—are embodied in a complex and, it must be said, often zigzagging and sometimes stodgy account with much extraneous information. Fergusson reaches back in time to the Depot of Military Knowledge of the war against Napoleon and mentions other British intelligence agencies, including the Naval Intelligence Department and the Secret Intelligence Service.

Organizational development and perceptions of threats to empire or homeland turn out, not surprisingly, to be closely connected. The establishment of the Intelligence Branch in 1873 reflected not only the initiative of an energetic officer, Captain Charles Wilson, and support from Secretary of State for War Edward Cardwell as part of his famous reforms but also the impact of Prussia's victories. The remaining years of the century, however, were, from a military intelligence point of view, dominated by the threat of Russia (in conjunction with France) to imperial, particularly Indian, security. The decline of the branch was reversed after the arrival of a new chief in 1886, partly on the basis of a mobilization plan that provided for "two army corps for overseas deployment" (p. 84). The available sources, Fergusson indicates, permit few generalizations about the nature of the activity of the Intelligence Division—as the Intelligence Branch was renamed in 1887—over the next decade, but "there was an unmistakable trend toward the use of covert or semicovert methods . . . in connection with officers traveling abroad" (p. 87). After the trauma of the South African War, the Esher reforms of 1904 brought into existence a directorate of military operations with responsibility for strategic planning and military intelligence. With Major General Sir James Grierson, a former military attaché in Berlin, as DMO, the directorate soon perceived Germany as the principal enemy and conducted its activities accordingly.

The chapters on field intelligence stress not only the revival of this activity during the South African War but also its continued emphasis in the years thereafter, again with a view to the coming war with Germany. An Intelligence Corps was activated in

August 1914 for service in the British Expeditionary Force.

Fergusson's book provides a valuable orientation to the subject. I doubt, however, that it will prove to be the last word on British Military Intelligence in this period.

PAUL GUINN
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ALEC CAIRNCROSS and BARRY EICHENGREEN. *Sterling in Decline: The Devaluations of 1931, 1949 and 1967*. Oxford: Basil Blackwell. 1983. Pp. vi, 261. £19.50.

Sterling's persistent weakness in the twentieth century contrasts sharply with its strength in the nineteenth century. This instructive volume describes the background events leading to the sterling devaluations of 1931, 1949, and 1967 and analyzes the consequences of each devaluation. Despite the differences in economic circumstances, both on the domestic and international fronts, there are some notable similarities, particularly with regard to economic policy formation. On each occasion the government of the day was extremely reluctant to accept the need for devaluation. Exchange rate adjustment, therefore, was never a carefully considered policy option but one forced on reluctant governments by the pressure of the marketplace. As Alec Cairncross and Barry Eichengreen pertinently observe, "What is interesting, therefore, is not so much the decision to devalue as the earlier, more deliberate, decisions, in 1930-31, in the spring of 1949 and in 1964-67, *not* to devalue" (p. 225). Yet the respective governments were equally reluctant to take domestic retrenchment measures to ease the pressure on the pound so that additional expenditure cuts had to be made after devaluation on all three occasions. Moreover, ministers were very unwilling to accept the market signals on their performance. "They overestimate their ability to employ political power to suppress or override market forces and too readily dismiss unwelcome trends in financial markets as the work of speculators" (pp. 225-26). It is true that markets are far from perfect and in the short term this may lead to overtracking, but in the longer term their verdict usually reflects fairly accurately the economic situation and ministers may ignore the signals at their peril.

What is surprising, given the steady deterioration in the competitive strength of the U.K. economy in the twentieth century, is how little discussion, and even less action, there was in official circles as to the alternatives to devaluation in terms of effecting an improvement in the economy's performance and efficiency. After all, devaluation is at best a temporary palliative that brings some relief, albeit variable,

to the external account, but it does nothing to change the fundamental parameters of the economy. It is tantamount to a temporary subsidy, whose effects diminish over time as adjustments take place, leaving the economy little better off competitively than before. The need for adjustment in the real economy, as opposed to the financial side, is at the root of Britain's problem, an issue somewhat neglected by the authors of this otherwise excellent comparative study of Britain's devaluations.

DEREK H. ALDCROFT
University of Leicester

RAYMOND A. CALLAHAN. *Churchill: Retreat from Empire*. Willmington, Del.: Scholarly Resources. 1984. Pp. xiii, 293. \$22.00.

Raymond A. Callahan has written an excellent synthesis of many of Winston Churchill's views on a variety of subjects. Among the areas given only rather fleeting attention, however, is empire, which is surprising because the subtitle of the book is "Retreat from Empire." Callahan makes no claim to originality in research, and indeed the book is based on heretofore published material. But many of the author's insights are stimulating, and the volume is written with verve and considerable literary dash.

An interesting prologue deals with the days surrounding the Dunkirk evacuation. Callahan is at his best when discussing and analyzing the infighting that took place during these crucial days in a somewhat divided cabinet. After this initial look at Churchill's character, Callahan turns back the clock to what he calls "The Long Preparation," beginning with the Dardanelles incident in World War I, the development of the tank as an offensive weapon, and Churchill's resignation from the cabinet to serve at the front. The treatment of Churchill's rehabilitation by Lloyd George, first as minister of munitions and then secretary of state for war and air, is followed by the first discussion of Churchill's views on empire.

Callahan points out that, although Churchill thought self-government appropriate for the colonies of white settlement, such as Canada, Australia, New Zealand, and South Africa, he advocated the continuation of direct British control over the dependent empire, and he distrusted the Arabs and supported Zionism. Churchill at first favored the repression of Irish nationalism but changed his mind under the influence of Michael Collins. His attitude toward the British connection with India never changed. As Callahan tells us, "Churchill's view of British rule in India as an unbreakable trust ignored not merely Indian nationalism but also growing uncertainty in Britain itself about the Empire" (p. 28). This stubbornly held view was one of

the few blemishes on an otherwise remarkable career, one of whose chief characteristics was adaptability and pragmatism.

Callahan next treats Churchill's inexorable drift into the wilderness and especially his conflicts with Stanley Baldwin, who emerges as a much wilier character than is usually depicted. Churchill's early identification of Hitler and his Germany as profound dangers to world civilization, of course, provided the key to his revival and eventual assumption of supreme power. But even after Churchill replaced Chamberlain as prime minister, he did not gain immediate control of the Conservative party machinery, and, despite the general appreciation of his unrivalled qualities as a war leader, he was never truly considered a party man but always as something of a maverick. Callahan's forte is describing the infighting that took place at the highest levels of British government.

In chapters 2 and 3, which cover some 175 pages, Callahan attempts to survey the entirety of Churchill's wartime experience: his strategic concepts and his relationship with his allies (particularly Roosevelt) and with his subordinates. Despite Callahan's real literary and intellectual skills, this task is probably not achievable in such brief compass, and the book develops a diffuseness and almost staccato rhythm. A final chapter deals with Churchill's fall from power and the postwar years with special emphasis on his literary efforts.

All in all, Callahan has written a highly readable book filled with well-documented insights into Winston Churchill's character, governing philosophy, and *modus operandi*. The discussion could certainly have been fuller in some instances, but, for readers interested in a brief yet thoughtful assessment of a very complex statesman, *Churchill: Retreat from Empire* is well worth consulting.

ROBERT A. HUTTENBACK
University of California,
Santa Barbara

JOEL KRIEGER. *Undermining Capitalism: State Ownership and the Dialectic of Control in the British Coal Industry*. Princeton: Princeton University Press. 1983. Pp. xii, 321. \$27.50.

This is, in essence, a polemic against the Weberian theory of bureaucracy, largely on the grounds that those who hold to the theory (as well as the bureaucracies themselves) tend to neglect the influence of changing class power. Joel Krieger also wishes to oppose what he calls the "Oxford school" of labor economists and the view of the Donovan Report, which was strongly influenced by it, to the effect that nationwide labor agreements are desirable: British practice and tradition, Krieger argues, are and al-

ways have been based on local practices and rules. The historical peg on which he hangs these arguments is the National Coal Board's introduction (with the consent of the National Union of Mineworkers) of the so-called National Power Loading Agreement (NPLA) into British coal mining in 1966 and its subsequent failure in practice.

The study is based on a detailed examination of ten collieries (curiously called "anthropologies") located in two coalfields, Durham and Nottinghamshire, with contrasting traditions and practices. The fieldwork in these collieries, five in each county, was extremely well done, though it is presented in a somewhat systematized fashion. The contrasts between the working practices and trade-union attitudes in these two counties, which are sharply delineated here, are in fact reflections of contrasts between two wholly different trade-union policies, one of which might be called the American-German-Scandinavian pattern, the other the British-Latin pattern. The former trade-union pattern, adhered to by Nottingham, calls for maximum productivity followed by bargaining for a share of it; the second pattern, the model for Durham, supports limiting output and then fighting for a bigger piece of the reduced cake. In addition, each of the ten collieries studied had its own peculiar traditional work and payments system, which survived the onslaught of the NPLA and forced its modification. Altogether, the author has no difficulty in proving his case for the significance of differences in long-standing regional and local practices.

He is less convincing in his other argument, postulating the inevitable failure of even a "perfect" bureaucracy. It may well be that the NPLA was defeated by the enormous differences in the geology between regions, collieries, even pits and seams rather than by the imposed uniformity as such. It may also be that the ambiguities of purpose underlying the nationalization of the British coal industry, aggravated by the contrary policies of alternating Labour and Conservative administrations, bore some responsibility for the weaknesses of all incentive wage schemes for coal mining in Britain. The wage agreement of 1966 is too egregious and complex an issue to decide debates about the functions of bureaucracies in modern societies.

SIDNEY POLLARD
Bielefeld University

PAUL THOMPSON *et al.* *Living the Fishing*. (History Workshop Series.) Boston: Routledge and Kegan Paul. 1983. Pp. xvii, 398. Cloth \$25.00, paper \$12.95.

There is an abundance of literature on the British fishing industry. As a major food source for the

island nation, it has received much attention in works of fact and fiction. This latest addition to the field, produced for the History Workshop Series, offers a comprehensive history of fishing over the past one hundred years. It covers several coastal communities, concentrating mostly on the eastern coast of England and the major Scottish fishing villages.

Those untutored in the subject will find the work informative and thoughtful. It is topically organized ("Capital and Labour," "The Economy and the Family," and so forth), with several case studies describing the character of selected towns. Paul Thompson and the other authors use written material and oral interviews creatively to make the work more readable. They have explored such subjects as religion, social structure, superstition, entrepreneurship, trade-union activity, individualism, community survival, and the effects of changing technology.

Interspersed throughout the book are case studies of several fishing communities. In some locations fishing was not the only source of income. Along the Moray Firth and on the west coast of Scotland, farming and fishing competed for labor, and sometimes fishing lost out. In the Shetlands, where the single purpose of "living the fishing" prevailed, communities tended to flourish in the face of economic setbacks, the arrival of big business, and even the caprices of the fish themselves.

The authors have used oral history throughout to provide the reader with the personal side of fishing. They chose respondents who gave both analytical and descriptive information and illustrated the large themes of the book. Too often, works that use interviews tend to become anecdotal and deflect the reader from the larger points and concepts being presented. The authors have relied mainly on primary and secondary written sources and have only integrated interview material to buttress their theses. And, where oral testimony has revealed significant interpretations, the authors have used it critically.

This work will be of interest to the sociologist, economist, anthropologist, historian, and other social scientists, as well as to the general reader. It contains helpful maps, a glossary of fishing terms, and good pictures. Its two-page index, however, is inadequate for a book that is so full of information and provocative ideas.

STEPHEN R. WARD
University of South Dakota

CHARLES CAMIC. *Experience and Enlightenment: Socialization for Cultural Change in Eighteenth-Century Scotland*. Chicago: University of Chicago Press. 1983. Pp. x, 301.

Partisans of the Enlightenment will welcome the central argument of this book, because it represents a hurrah for the enlighteners of eighteenth-century Scotland. Adam Ferguson, David Hume, John Millar, William Robertson, and Adam Smith are interpreted as the harbingers of modernity who brought about the emergence of "modern cultural orientations" by rejecting the sociological models of "dependency and particularism" that permeated their Calvinist environment and championed in their place the models of "independence and universalism."

Any reader and admirer of the five Scots will readily agree with Charles Camic, and this reviewer takes pleasure in seeing the work of Peter Gay receive the explicit confirmation and approval it deserves, although almost twenty years after the publication of his bold and brilliant volumes on the Enlightenment. As far as the historiography of the *high* Enlightenment is concerned, this monograph is correct but late.

Trouble arises, however, with the methodology. There is almost no discussion of the world of the Scottish Enlightenment. In fact, the author seems to deny that there was a Scottish Enlightenment outside the work of the five. No mention, for example, is made of the debating societies of mid-century where there was a concerted effort to bring Scotland into the mainstream of the European Enlightenment, nor is sufficient background provided for Robertson's attempt to reform his church or university. It would be wrong to insist that all students should concern themselves with the social history of ideas, but, when the tensions that existed between reactionary and progressive currents are ignored, the result is not only unhistorical but unconvincing as well. Camic rests his case for the enlighteners on the process of socialization—family life, educational experience, and professional setting. But for all five the information in these areas is sketchy at best, and Camic bases his account on dated and flimsy secondary sources. It is true that we do not know enough about the lives and struggles of the laconic Scottish enlighteners, but it is discouraging to see these lacunae turned into theoretical certainties, especially when the thesis is on the side of the angels.

VICTOR G. WEXLER
*University of Maryland,
Baltimore County*

PETER L. PAYNE. *Colvilles and the Scottish Steel Industry*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xxi, 458. \$89.00.

In the scramble of Scottish producers to survive in the revolutionized steel industry at the end of the 1870s, the family firm of David Colville emerged by

1914 as one of the largest enterprises in the United Kingdom. As with most British steelmakers, the firm subsequently passed through a series of amalgamations and private efforts at rationalization, until it became a reluctant component of the nationalized industry in the 1960s. Its story, therefore, is at the heart of the modern British industrial experience.

Neither a commissioned nor a conventionally celebratory work, Peter L. Payne's book is a hard-headed analysis of the recurrent problems of heavy industry in Scotland. It is a specialized and highly professional form of modern business history and is based on an almost embarrassingly plentiful archive of commercial and technical transactions, which stretch across a century of industrial evolution. It is essentially a tale of many efforts to create defensive oligopoly arrangements between industrial rivals in the face of interregional and international competition. Consequently, much of Payne's book is a detailed and clear-minded diplomatic history of negotiations—a dialogue between Colvilles and the rest of the Scottish steel industry. Most of the book addresses organizational questions in a context of competing desires for autonomy and amalgamation until the British government eventually imposed a nationally integrated policy on the entire industry. There are few concessions to the general reader in this account, few entrepreneurial anecdotes, but the Scots are fortunate to possess so penetrating a study of this most awkward industry. It should give greater clarity and perspective to the options it is likely to face in the future.

Colvilles's story is one of relative success in an industry and economy that have struggled, with diminishing confidence, to retain their shares of British and international markets. Payne provides a narrative of technological innovation and intra-industry negotiation. Though he presents considerable statistical data, he avoids anything like an econometric analysis of the causes of differential productivity. He tells us that Colvilles was a good employer and achieved marked social cohesion, but he has chosen not to study labor relations. Surprisingly also, Payne allows only a few fleeting remarks—these of great interest—about managerial psychology and the separation of ownership from control. Nor does he explicitly relate this microcosm to the broader questions of the retardation of British industry.

The somewhat abstracted quality of this study, removed from the sweat and anxiety of the industrial community, may be contrasted with its trenchant narration of the radical surgery and reconstruction that was demanded of the steelmakers along the road toward regional and national integration. The awful complexity of such industrial monoliths, and the inevitable political pressures under which they labor, are eloquent reminders of the

methodological difficulties that face modern economic historians. Among many important themes, the author demonstrates the manner in which steel became hostage to the shipbuilding industry and to the financiers, describes its remarkably vigorous condition in 1939, shows the power of local inertia and the missed opportunities for reorganization, and repeatedly emphasizes the conflict between private and public perceptions of the best strategy for industrial progress. Payne is especially clear on the problems of matching the latest technology with the capacity of the market to absorb rapid augmentations of output. On nationalization and its unhappy financial results, the associated convulsive administrative reorganization, and the apparent exacerbation of regional rivalry within the steel industry, this book has many illuminating thoughts. Most of all, Payne's determined realism will help dissuade fellow historians and others from facile diagnoses of the problems of structural change in the modern industrial economy. But it would have helped had he located his work more directly in the mainstream of that debate.

ERIC RICHARDS
Flinders University

JOEL MOKYR. *Why Ireland Starved: A Quantitative and Analytical History of the Irish Economy, 1800–1850*. Boston: George Allen and Unwin. 1983. Pp. x, 330. \$29.95.

The only nation in the world to have lost half its population since the mid-nineteenth century, Ireland continues to attract scholars who want to know the reasons why. In particular they seek to understand the nature of the potato monoculture, the extent of poverty, and the causes of the Irish diaspora before and after the great famine of 1845–48. Why so many thousand young Irish men and women forsook rural poverty at home for urban squalor abroad remains one of many unresolved questions about Ireland.

However remote in time they may be, the horrors of the famine years still stir deep anger among people of Irish Catholic descent, most of whom were brought up on stories about people reduced to eating grass or boiled nettles, deserted villages, soup kitchens, mass graves, and coffin ships bearing diseased emigrants to their deaths at sea or in the ports of the New World. These collective memories continue to provide live ammunition for Irish nationalists who blame all of Ireland's woes on "perfidious Albion," or the British government. To more extreme patriots, the Whig rulers of the latter 1840s were guilty of nothing less than genocide because they wanted to reduce the Irish population to a manageable or economically viable level. According

to the "holocaust school" of Irish history, Lord John Russell and his colleagues deliberately turned their Sassenach backs on the starving peasantry, leaving them to die by the thousands. Besides followers of Sinn Féin and the IRA, this indictment enjoys the support of an eminent English historian, A. J. P. Taylor, whose review of Cecil Woodham Smith's *The Great Hunger* (1962) began with the observation that "all Ireland was a Belsen" during the famine years.

In sharp contrast to the genocidal school, most Irish historians nowadays regard government policy during the famine as falling somewhere between benign and malign neglect. Aware of the British Treasury's parsimonious attitude toward relief efforts in Ireland, they tend to emphasize British errors of omission rather than commission. Although Joel Mokyr seems to come closer to the "malign neglect" model in his conclusion, his book is not really about the famine per se, nor does it delve into the inadequacies of famine relief. A keen practitioner of the "new economic history," he sets out to explain why Ireland did not achieve the kind of economic growth that would have averted the catastrophic effects of three successive failures of the potato crop. Anyone familiar with Mokyr's six or seven articles on pre-famine Irish society and economic behavior will know what to expect from his book, for in both cases he explores the structural causes of Irish economic "backwardness" at a time when Britain and Western Europe were advancing steadily toward an industrial and famine-free existence. (Readers in need of a good guide to Mokyr's principles and methods of modern political econometrics should read Liam Kennedy's summary review of the articles in *Irish Historical Studies*, 91 [May 1983].)

As befits a hard-core econometrician, Mokyr tries to measure almost everything in sight. Besides such obvious variables as birth, death, nuptiality, and fertility rates, he also quantifies agricultural productivity, land tenure patterns, per capita wealth, agrarian crime, landlord absenteeism, emigration, and famine-related deaths. All these and more are subjected to regression by the computer in his ambitious effort to place the history of pre-famine Ireland on a more scientific footing.

Regrettable as it may be, this reviewer did not learn enough algebra at school to appreciate the nuances of correlation coefficients, generalized least squares, or Cobb-Douglas production functions. Nevertheless, the fancy mathematical modeling coexists with a good deal of qualitative argumentation based on material supplied by witnesses before various select committees and commissions of inquiry as well as contemporary observers of the Irish scene. Whatever the shortcomings of the data being regressed and however much of the sophisticated methodology may be lost on nonnumerate readers,

there can be no doubt that a bright and bold mind is hard at work in this book, wrestling with some of the most complex and intractable problems in modern Irish history.

Mokyr's study resounds or rumbles with the demolition of old models or theories of Irish underdevelopment, and his brand of revisionism by regression is bound to provoke some spirited exchanges among advocates of the new social as well as economic history. Those who do not belong to this select fraternity, however, should not be put off by all the elaborate equations and neopositivist premises, for Mokyr has a way of asking tough and important questions that more timid historians prefer not to pose. He has organized his book around a series of conventional hypotheses or theories about Ireland's failure to industrialize, which range from lack of natural resources to British political and economic policies, social factors, Malthusian theories of overpopulation, insufficient capital formation, land tenure, and loss of talent and money through emigration. Mokyr uses government data, especially census reports and parliamentary inquiries, to evaluate each of these potential explanations in turn. Unfortunately, he shares the prejudice of some social scientists who regard archival or manuscript sources as unnecessary or unhelpful. There is no mining here, for example, of the rich, if scattered, veins of social and economic materials found in the estate papers collected in Dublin and Belfast. Closer acquaintance with these sources might have made the author warier of accepting the often impressionistic or prejudicial testimony given to poor law committees and royal commissions, especially when witnesses were not testifying under oath or subject to rigorous cross-examination.

Armed with his computer, Mokyr advances fearlessly across the often disputed terrain of Irish economic and demographic history, striking blows to right and to left at rival theories or hypotheses. For someone so devoted to number-crunching, he is surprisingly ready and eager to quantify the evidence of witnesses who dealt in such terms as "much," "many," "some," or "a few." Reliance on such soft or mushy data hardly inspires confidence in his estimates of economic deterioration, monetization, consolidation of holdings, absenteeism among landlords, and agrarian crime. Indeed, the problem of crime or Irish rural violence requires far more archival research, especially in the State Paper Office, than is to be found in chapter 5. Mokyr's assertion, moreover, that most farms were let on "long leases" rests on a selective sample of "lots" or groups of tenancies sold in 1850 in the Incumbered Estates Court. That this sample is biased is arguable because it derives solely from *insolvent* estates, many of whose owners may have been forced to sell precisely because they had not managed to convert

their low-rent leases to yearly tenancies at a higher rent. Only a much larger sample of solvent as well as insolvent "estates," rather than "lots," can provide the kind of firm foundation that is needed for such generalizations. To put the matter another way, there are times when Mokyr's statistical manipulations are far too sophisticated for the kind of crude or flawed data he has collected in his journey through the British parliamentary papers.

No short review can do justice to so argumentative and carefully calculated a study. Virtually every page contains food for thought or material worth discussing at length, and the book raises questions about the causes of poverty not just in Ireland but the world over. For all of its heuristic qualities, however, there is disappointment in store for those who follow Mokyr's path of exploded myths or demolished hypotheses to the end. Each chapter serves to raise the reader's expectation of a grand or climactic answer to the Big Question—why did Ireland fail to industrialize? And yet there is no "big answer" in the conclusion. Instead of a summary explanation, Mokyr arrives at much the same conclusion advanced by T. S. Ashton in his minor classic *The Industrial Revolution* back in 1948. Ashton, the apologist of industrialization, pointed to Ireland as a terrible example of what can happen to a country that fails to solve the basic problems of feeding, clothing, and employing a burgeoning population. Once Mokyr has discarded all the standard explanations of Irish poverty—lack of capital, political subjugation, the crushing of cottage industries, the disadvantages of the potato, emigration, and so forth—what is left? The answer he gives amounts to another set of questions. Perhaps, he reflects, the right question to ask is not why Irish poverty but why Western European prosperity? And then in a profoundly agnostic aside he observes: "The causes for the wealth or poverty of nations are in some sense not only unknown but unknowable. . . ." (p. 293). Fair comment, but why wait so long to tell the reader that the question informing the entire study cannot really be answered? The many insights in this work notwithstanding, some readers may experience pangs of hunger—to use the obvious metaphor—after finishing this rich econometric fare, wishing that the menu had included more explanatory meat and fewer methodological potatoes.

L. P. CURTIS, JR.
Brown University

DEIRDRE MCMAHON. *Republicans and Imperialists: Anglo-Irish Relations in the 1930s*. New Haven: Yale University Press. 1984. Pp. x, 340. \$30.00.

In this first major study of a highly controversial period in Anglo-Irish relations, Deirdre McMahon

traces the tortuous course of events that followed Eamon de Valera's accession to power in 1932. De Valera's insistence on withholding land annuity payments and abolishing the oath of allegiance to the crown initiated a six-year struggle with Britain marked by acrimonious exchanges and economic warfare. After de Valera had scrapped the Anglo-Irish treaty of 1921, severed almost every link with the British Commonwealth of Nations, and made the Irish Free State a republic in all but name, the British finally gave way. In 1938 a series of agreements settled all outstanding disputes, with the notable exception of partition, on terms very favorable to the Irish. The return of the Irish naval bases that Britain had retained under the 1921 treaty made possible Ireland's neutrality in World War II.

McMahon's work is meticulously documented, well organized, balanced in judgment, and highly readable. Her discussion of issues and personalities is incisive and illuminating. Although she readily acknowledges de Valera's domination of Irish policy and generally masterful political tactics, she is not uncritical of "the Chief," pointing out his inflexibility on certain issues and his occasional lapses, for example, the affair of the Governor-General. In comparison, the British fare badly. While J. H. Thomas is treated less harshly than usual and Neville Chamberlain is given due credit for his pragmatic conversion to conciliation, only Malcolm MacDonald, Thomas's successor as dominions secretary, and a handful of Whitehall officials are given high marks for their realistic approach to Irish problems. McMahon argues convincingly that most British ministers were not only woefully ignorant about Ireland but also blind to major changes in the Commonwealth after 1921. The British were correct in viewing de Valera's opening attacks on the treaty settlement as the thin end of an Irish republican wedge, but for too long they refused to realize that they could not prevent the restless Irish dominion from going its own way. Old-fashioned coercion was out of the question, economic sanctions were a two-edged sword, and arguments about Irish legal and moral obligations were shaky to say the least. After all, what right did Britain have to impose its constitutional views on another member of the Commonwealth, a free, voluntary association of equal and autonomous states? In the end, British unwillingness to face unpleasant facts may well have weakened the Commonwealth as much as de Valera's determined efforts to assert Irish independence.

Because she was not allowed to consult some official records in Dublin and Belfast or de Valera's papers, McMahon's work is not definitive. But her skillful use of a wide range of British, Irish, and American sources has produced a first-rate study in

diplomatic history that should be warmly welcomed by Irish, British, and Commonwealth historians.

JOSEPH M. CURRAN
Le Moyne College

ERICA HARTH. *Ideology and Culture in Seventeenth-Century France*. Ithaca: Cornell University Press. 1983. Pp. 333. \$35.00.

Seventeenth-century France has often been studied as if it were a turning point between medieval and modern and aristocratic and bourgeois societies and cultures. A structural shift has often been perceived that links social and political change to dominant cultural and philosophical movements. The aristocracy is seen either to have given way to or to have been defeated by (the difference is of fundamental importance) the rising bourgeoisie and the modern state. The ethics expressed by Malherbe and Corneille appear as a swan song of the chivalric mind. The human qualities of Racine's and Molière's heroes and heroines signal the articulation of a new bourgeois reality.

The book under review is a historical and philosophical analysis in this tradition. For Erica Harth the fundamental characteristic of aristocratic culture was its creation of moral-social truths by mimetic strategies. The *ut pictura poesis* mode of generating truth in history, the portrait (in *L'Astrée*, in particular) constituted a gigantic myth that would slowly cease to be believed. The publication of pseudo-memoires, political testaments, and the *nouvelle* responded to a search for truth more in accord with social realities. Still later in the seventeenth century, truth came to be discerned more fully in the works of natural science, in travel literature, and in secret histories.

The author assumes something more than a historian's authority when she discerns where and to what degree seventeenth-century readers found truth. Sometimes this authority appears to rest on her appreciation of the numbers of readers of a particular genre, at other times on her judgment of the intellectual sophistication or professional limitations of writers (Boileau and Racine are demeaned again, because they were "poets" rather than "historians" when commissioned to write history), and at still other times on a self-evident reasoning grounded on the assumption that the creation of new genres is prompted by an almost ineffable quest for truth. Only in a few great paintings does truth transcend these shifts from aristocratic to bourgeois truth, as Marx himself observed.

The consequences of following recent literary, rather than older liberal (Augustin Thierry) or Marxist, concepts about the bourgeois are apparent in the definitions of reader and author. In the

liberal histories of the rise of the bourgeoisie, a libertarian and participatory force was discerned that increased the power of the readership. Thus, reading may be interpreted as a search for knowledge about man, society, and the world that enhances powers to discern truth. In Harth's work, the bourgeois is described as a producer of texts and a literal reader who is incapable of abandoning the search for truth but never acquires any. The truth in bourgeois literature is not examined here, nor is the problem of whether it too depended on mimetic strategies for conveying meaning. The works of Eric Auerbach and Paul Bénichou are not cited, perhaps because they offer interpretations of the nature and complexity of mimetic bourgeois truth. The old bourgeois-state alliance against the aristocracy also has disappeared, and with good reason, since it posited a rise to power of the bourgeoisie that depended on the quest for liberty, the power to interpret, and participation in political life.

By discerning bourgeois truth primarily in the natural sciences and travel literature, Harth depoliticizes the bourgeois. But was there such a shift from one mythical truth system to another that resulted also in a change of fields of knowledge in which truth came to be discerned? The author's claim prompts recollection of facts that do not square with her interpretation. La Popelinière, that eager imperialist student of the Third World of Australia in the 1570s, would have been surprised by Harth's claims after waking up to read Foigny.

OREST RANUM
Johns Hopkins University

CHERYL B. WELCH. *Liberty and Utility: The French Idéologues and the Transformation of Liberalism*. New York: Columbia University Press. 1984. Pp. viii, 289. \$30.00.

This book treats thoroughly the changing emphasis of liberalism in France from the aristocratic eighteenth century to the bourgeois nineteenth century. Its style, clear and dense, harmonizes with a train of thought in which Cheryl B. Welch reviews and summarizes at least as much as she discusses and judges the idéologues' essential ideas.

In a concise introduction, the author states her two aims: to interpret the idéologues' social and political ideas and to target discussion mainly on their theories in order to elucidate "the problem of how and why the fusion of utility, liberty, and democracy developed in the language of political theorizing" (p. 2).

The first chapter, which is largely historical, traces the thought of leading idéologues (such as Volney, Condorcet, Sieyès, Destutt de Tracy, and Daunou) on natural rights and the proposed French consti-

tution. Their common concern was to develop a liberal, utilitarian regime that both respected natural rights and conferred political and economic power on the bourgeoisie. Influential in educational and academic circles under the Directorate (October 1795–November 1799), they were reduced to powerlessness under Napoleon.

The historical aspects reappear in the sixth and last chapter, "Diaspora and Dissolution," which focuses on surviving idéologues (Daunou, Tracy, Say, Constant), their followers (Thurot, Laromiguière, and Maine de Biran), and a few younger disciples. After their suppression by the Jacobins and by Napoleon, the idéologues were barely tolerated under the Bourbon Restoration. Under the July Monarchy of Louis-Philippe, a few of their political and economic positions were blended into Saint-Simonianism, the positivism of Auguste Comte, and the utopian socialism of Charles Fourier.

Welch concludes that idéologue liberalism should be understood "in the context of political and constitutional crises," because it makes clear "the nineteenth century's commitment to constitutionalism" (p. 189). Yet as much by its basic lack of coherence as by external adversaries, *idéologie* failed to impose its new synthesis, even though it assisted in recasting political discussion in France. Utility alone justified government, which in turn fostered security and laissez-faire, provided that civil equality did not fall victim to cartels and corporate privileges. Among idéologues, Tracy and Daunou upheld a liberal constitution to safeguard natural rights, to apportion authority, and to establish the accountability of public officials. Their followers, however, abandoned reliance on constitutions, because they witnessed the confrontation of popular expectations, many of them illusory, with the reality, frequently painful, of the industrial revolution.

ALFRED J. BINGHAM
University of Maryland

ELLIE NOWER SCHAMBER. *The Artist as Politician: The Relationship between the Art and the Politics of the French Romantics*. Lanham, Md.: University Press of America. 1984. Pp. viii, 248. Cloth \$22.50, paper \$11.75.

An adequate definition of romanticism has long eluded scores of established scholars. In 1924 Arthur O. Lovejoy warned that there was not one but a plurality of romanticisms whose manifestations varied according to genre, culture, and period. Not until the more recent syntheses by Morse Peckham (1951, 1961), Henry Remak (1961, 1972), and René Wellek (1949, 1963) have very clear boundaries circumscribed this problematic intellectual and artistic phenomenon in early nineteenth-century Europe. All historians today remain very much in their

debt. Ellie Nower Schamber, however, has shunned Lovejoy's caveat and the important work on romanticism since then. Rather, the author has based an entire book on an excessively narrow definition, even for France, that focuses only on "those artists who applauded the innovations of Victor Hugo's play *Hernani*" (p. 3). The problems this poses to the book's analysis ultimately undermine its major contentions about the relationships between art and politics.

Schamber argues that romanticism "expressed precisely the dilemma of the dispossessed noblemen" attempting to maintain a rapidly eroding status in an increasingly open social and political world (p. 12). "Thus," Schamber writes, "art provided a psychological outlet for the Romantics, as well as a means of economic sustenance and a stepping stone to a political career" (p. 13). The author studies the effect of this historical dilemma on most supporters of *Hernani*—an odd assortment including such diverse figures as Chateaubriand, Constant, Lamartine, Hugo, Dumas père, Vigny, the adherents to the *Petit Cénacle*, and the *Bouzingos*. Schamber claims to document the romantics' "unified world view which unmistakably grew out of the social context of their lives" (p. 193). This "coherent Romantic philosophy" of moderate liberalism, in both politics and literature, apparently was derived from the distinct historical tension actually experienced by all *déclassé* artists in the first half of the nineteenth century (p. 13).

Unfortunately, Schamber's close attention to the voluminous memoirs and correspondence of these greater and lesser lights in French literary history fails to redeem the analytical misconceptions of the book. Schamber not only grossly oversimplifies romanticism but also fails to examine in any detail the relevant romantic literary texts—crucial sources for a study of this sort. Nor does *The Artist as Politician* fully come to terms with the social context of the writers it discusses: it never occurs to the author that nearly all aristocratic and bourgeois artists in France at the time, romantic and neoclassical alike, suffered the same difficulties in securing acceptable careers. Moreover, contrary to the author's assertions, not all romantics shared the same political principles. I cannot imagine what politics Chateaubriand the temperamental royalist had in common with George Sand the sentimental republican.

In short, Schamber has written a book that is scrupulously researched but conceptually flawed. This problem could have been checked by wider and deeper reading of both the relevant scholarship on romanticism and, especially, the romantic literary works themselves. Schamber was not well served either by dissertation readers or by the publisher's

referees, all of whom share responsibility for the publication of this book.

JAMES SMITH ALLEN
University of Pennsylvania

ROBERT GILDEA. *Education in Provincial France, 1800–1914: A Study of Three Departments*. New York: Clarendon Press of Oxford University Press. 1983. Pp. viii, 408. \$55.00.

Robert Gildea has written a book that is in parts unique and on the whole well worth having. He has studied education under different regimes for three departments—Ille-et-Vilaine, the Gard, and the Nord—all peripheral, all strongly Catholic in at least some swaths, and all intensely regional. His thesis, clearly stated at the outset but not always sustained, is that Parisian educational changes did not root the same way everywhere but were distorted by provincial conditions and limited in acceptance by local variations. There was a constant dialogue between Paris and provinces and an inherent geography of attitudes (Gildea's terms). In the Gard an excellent climate for anticlericalism existed, but only in Protestant-dominated areas. In Flanders linguistic separateness and stubborn Catholicism blunted Parisian schemes. In Brittany the *chouan* and Catholic tradition won out just about everywhere, making lay institutions circumspect and rival Catholic ones robust.

Strengths of the book include some new chronological orientations, such as considering 1860 a watershed: "Only after 1860 did public education become increasingly identified with an anticlerical position" (p. 367). One is tempted to quarrel, but some of the evidence makes sense. Gildea sees the last ten years of the Second Empire as foreshadowing the republican onslaught after 1879 and makes other provocative generalizations, for example, that the two *jeunesses* were less *laïques* and Catholics than boys and girls. Sensibly, he reverses the trend to frame ideological currents in material terms. Religious quarrels, at least until 1890, were not mere smokescreens; the Catholic church was still strong, and in one chapter the author identifies a rival "Republican Church," and he is right to do so. Toward the end of the book he pulls back, noting that from 1880 people only wanted opportunities for literacy and advancement, not the "sour-faced preaching of republican morality" (p. 368). In fact, almost no Frenchmen before 1914 lacked belief, whether in Catholicism or laicism or some variation thereof. Socialists like Jaurès were also "preachers of republican morality."

Specialists will learn a lot about lay schoolmasters, especially the limits of their effectiveness, although in spots Gildea duplicates the work of predecessors.

He has good material on school libraries and the geography and efficacy of adult courses. His pages on the Catholic "Battle of the Books" of 1908–09 fill a gap. He includes many tables, which show, for example, literacy by occupational groups, and many useful maps. He clearly identifies the many kinds of schools in France and offers definitions that help students: for example, liberty of education—"the right of any qualified and decent individual to open a school" (p. 102). He correlates school attendance with booms and slumps and shows how the need for catechisms and First Communion kept pupils in class. In sum, he can teach any specialist in primary, secondary, or technical French education many things; only the new university system of Liard (Rennes and Lille) is slighted.

Sometimes Gildea throws out too many names of officials and schools. The book is also fairly predictable in style—I like it best when he makes daring statements or analogies to break up narrative. For instance, on the problem of local tongues he remarks that "teaching French to children in primary schools could be as difficult as teaching Latin to the pupils of secondary schools" (p. 223). More anecdotes would also sharpen our perception of problems like *déclassement*. In one he quotes the son of a shopkeeper who became a doctor: "Until the age of fifteen I had a good deal of enthusiasm for commerce but the study of classics made me blush to help my parents in the shop" (p. 203).

Gildea makes one large error, noting a "depression" in France from 1873 to 1895. There were slumps, of course, but this was a great age of material upliftment in the countryside, as Roger Thabault and Eugen Weber show (Gildea, however, uses *Peasants into Frenchmen* sparingly and ignores *Mon Village*). Also, in the bibliography he calls Weber "Eugène Weber," which is *beau*, but not correct. A G. H. Weiss, twice cited, should be John Weiss. And in the text the Forest of Paimpont should be Paimpont.

In general, the archival research is dazzling, but the book relies too much on one kind of source. Gildea does not connect sufficiently with other *laboureurs* in the field.

But, to reiterate, this is a very solid monograph with parts that are original both in perspective and detail. No student of French education or regional life of the nineteenth century can afford to ignore it.

BARNETT SINGER
Victoria, B.C.

JEAN-MARIE MAYEUR. *La vie politique sous la troisième République, 1870–1940*. Paris: Seuil. 1984. Pp. 445.

Jean-Marie Mayeur (of the Université de Paris IV) has distilled a quarter-century's thinking and writ-

ing (seven previous books) about the Third Republic into this analytical study of politics and political life in France between 1870 and 1940. His mastery of a large body of research by French, English, Canadian, and American scholars is complete. Within the narrow limits he has set for himself, Mayeur establishes a new standard of synthesis.

The preponderance of the legislative houses was the keystone of the Third Republic, and Mayeur provides a subtle and well-developed analysis of the relationships between the Chamber of Deputies and the Senate and between the two houses and the president of the republic. Skillfully integrating new research on the formation of organized political parties in France, he argues that the long delay in this development, which encouraged the independence of legislators, led to the identification of causes with individual politicians and, ironically, to the *pouvoir personnel* the republicans feared and sought to limit. Mayeur insists that the "clerical line" divided the moderate left from the moderate right not only during the early years of the republic but also into the late 1920s. He urges a close examination of the *conseillers municipaux* and *conseillers généraux*, the *petits notables* in provincial France, where the battle for a laic republic was fought and won.

Mayeur provides shrewd *aperçus*. The Seize Mai episode was no more than "a struggle of the past led without great conviction" (p. 67). The easing of laws regulating cabarets stimulated the creation of republican foyers. Bastille Day and "Marianne" were symbols of laicism, not merely of republicanism. Boulangism was a crossroad where monarchism was halted but where socialism and antiparliamentary nationalism were given impetus. Camille Pelletan's genial incompetence led Alain (Émile Chartier), the philosopher of the Radical party, to call him the ideal cabinet minister. Far from undermining the French left, the Congress of Tours strengthened it by enabling a moderate center to ally with a socialist party freed from its revolutionary wing. Paul Reynaud responded to the military disaster of May 1940 by recalling the surviving heroes of France's last victory—Philippe Pétain, Georges Mandel, and Maxime Weygand. In part, the failure of the Third Republic in the 1930s was a collapse in France of the values that had been the regime's framework: faith in the school, in democracy, in the nation (*patrie*) itself.

Useful appendixes (a list of the presidents of the republic, the Chamber of Deputies, and the Senate; the constitutional laws of 1875; and nine charts of voting behavior) complement the text. Although well chosen, the bibliography is severely limited to political history.

BENJAMIN F. MARTIN
Louisiana State University

GORDON WRIGHT. *Between the Guillotine and Liberty: Two Centuries of the Crime Problem in France*. New York: Oxford University Press. 1983. Pp. ix, 290. \$19.95.

Gordon Wright is widely admired for his knowledge of modern France and his gift for synthesis when he writes about it. Both of these qualities are on display in his latest book. Readers will also have the pleasure of learning what happens when a former president of the American Historical Association sets out to satisfy a personal "curiosity born of ignorance" (p. vi) about the past. What better reason is there for a historian to get to work?

Family discussion about American prison reform at the time of the Attica riot of 1971 first stirred Wright to wonder about the case of France. Unable to attract a graduate student to the project, he resolved to take it on himself, only to discover that "a whole cluster of historians had just descended on the crime problem, like flies attracted to carrion." We can be grateful he persisted, for his chronological review of the debate over the criminal-justice system in France from the eve of the Revolution of 1789 to the first days of the presidency of François Mitterand is the only general treatment of the subject in any language. *Between the Guillotine and Liberty* is less a book about Papillon than about the Frenchmen who sent him to Devil's Island and their opponents who thought he could be rehabilitated in a well-run prison. Assembling his evidence from administrative and judicial records, parliamentary debates, commission reports, books, newspapers, and articles, Wright presents a sobering account that is certain to interest anyone concerned with the law, politics, and the treatment of criminals in modern society.

"It is usually easier to narrate what happened than to explain why it happened," Wright states (p. 16). His own view is that reformers in every regime since the eighteenth century have been inspired by a mixture of humane sentiments and utilitarian values—Beccaria, he reminds us, wanted to substitute perpetual slavery for the death penalty. Acknowledging that this thesis represents a "return to an older orthodoxy" (p. 22), Wright presents it as a polite, yet distinct, challenge to what he describes as the "ingenious" and "complex" argument made by the late Michel Foucault in *Surveiller et punir: Naissance de la prison* (1975). Similarly, he rejects Marxist explanations of bourgeois reform as "reductionist" and "more plausible than persuasive." There is a sincere and practical tone throughout this book.

Three conclusions stand out from Wright's survey, at least for this reviewer. First, France's historical experience with criminal justice "parallels that of other Western nations" (p. 219), with the obvious exception that the last prisoners were not repatri-

ated from its penal colonies until 1946. Second, the case of France suggests that all efforts at reform are highly susceptible to shifts in political mood. Public perception of a rise in crime rates, for example, can destroy the work of a commission of experts. Finally, the official position on law and order taken by its many regimes seems to confirm much of what we have already learned about them in other regards. The constitutional monarchy made sweeping changes after 1789, many of which were sacrificed by the troubled republic. Both Bonapartes were authoritarian, the Bourbons were less so, and the July Monarchy talked about reform so long that it fell before doing much about it. The visionary changes of the Second Republic were swept away after 1851, the governments of the early Third Republic were conservative in liberal garb, and the Popular Front tried to close the notorious *bagnes* before departing office. Vichy was Vichy, the Fourth Republic did more than it is given credit for, and the Fifth Republic talked tough at the start, only to be confronted with the prison rebellions of the 1970s. The abolition of the death penalty, the reduction of prison population, and the overturning of the Peyrefitte "law of security and liberty" by the socialist supporters of Mitterand constituted "a reversal . . . almost without precedent in France" (p. 216) but also angered the right and frightened ordinary citizens. History, it seems, is always cut from whole cloth.

ROBERT BEZUCHA
Amherst College

LESLIE DERFLER. *President and Parliament: A Short History of the French Presidency*. (Florida Atlantic University Book.) Boca Raton: University Presses of Florida. 1983. Pp. ix, 286. \$20.00.

This well-written survey of the French presidency from Adolphe Thiers to François Mitterand fills a surprising and significant gap in the historiography of modern France. The paucity of archival sources makes the study of the inner workings of the French government a daunting task, but Leslie Derfler brings a commendable ingenuity and enterprise to his research. Happily, he does not force what evidence is available into any preconceived mold, and the political evolution of the presidency is presented as having its own logic and its own imperatives. When relating the presidency to the underlying forces shaping French society, Derfler appears to follow the polycasual economic determinism of Shepard Clough.

After France's first unhappy experiment with a popularly elected president in 1848, French republicans remained understandably wary of the presidential office. Derfler shows how the presidency of

the Third Republic was designed by its mostly monarchical authors to be a powerful office, but how in practice it was weakened either unintentionally (by Patrice MacMahon) or deliberately (by Jules Grévy). Only in limited ways could a president balance the power of the parliament, and the few who tried to play a more important role were inevitably frustrated. At various times during the Third Republic different groups took up the cause of a strengthened presidency, but without much effect.

Derfler shows how, after the ambiguous experience of Marshal Pétain's Vichy regime, the formation of the Fourth Republic revealed little change in the balance of forces within French society. Thus, a presidency remained necessary, but one with limited powers. Vincent Auriol made as much as possible out of the office, but his *de facto* influence did not pass to his successor, René Coty, whose most important action was to engineer the succession of Charles de Gaulle in 1958.

The weakness of the executive power under the Third and Fourth Republics persisted, Derfler argues, because it was consistent with the needs of French society. The growth of the postwar welfare state and the increased role of the government in economic planning made administration more important than legislation, but it took the Algerian crisis to produce a constitutional revision in the role of the president. Indeed, the presidency became more powerful than intended by the constitution of the Fifth Republic, not so much because of the general's personality as because of the development of a presidential majority in parliament. This unexpected evolution enabled de Gaulle's powers to be transferred to his less charismatic successors. What will happen if a parliamentary majority and a president are strongly opposed has not yet been tested.

Derfler's biographical sketches of the presidents, from the most obscure journeymen politicians of the Third Republic to the more public figures of recent experience, are an especially valuable feature of this work. Moreover, these vignettes are not isolated oases in a desert of formal constitutional history, for Derfler gives us the living story of the presidency through the day-to-day struggles of its tenants to give meaning to the office.

WILLIAM LOGUE
Northern Illinois University

ZEEV STERNHELL. *Ni droite, ni gauche: L'Idéologie fasciste en France*. Paris: Seuil. 1983. Pp. 407. 99 fr.

Zeev Sternhell demonstrates convincingly that fascism was not simply imported into France or vaguely modeled after Italian fascism but was strongly rooted in a French political and intellectual

tradition dating back to Boulangism. Prewar writers like Maurice Barrès and Georges Sorel created not only an ideology but also a vocabulary and style that was later echoed by French fascists of the 1930s.

Less convincing is Sternhell's insistence that "authentic" French fascists, although presumably "neither left nor right," were socialists who were "violently anti-bourgeois and anti-conservative" (p. 121). "In revolt against bourgeois society," they desired, like the Marxists, the destruction of the old order. As early as 1911, syndicalists of the Proudhon Circle sought to resurrect "an old French socialism" that was much more revolutionary than the social democracy of Jean Jaurès and Eduard Bernstein, who had compromised with the bourgeoisie by participating in political democracy—the bourgeoisie's best weapon. In the 1930s neosocialists like Henri de Man and Marcel Déat continued this "post-Marxist revisionism" by undertaking a spiritual and ethical renewal that rejected both bourgeois and Marxist materialism. For Sternhell, "It is always the revision of Marxism that constitutes the most significant ideological dimension of fascism"; "one can write the history of fascism as the incessant effort to revise marxism" (p. 34). Indeed, he argues, all authentic fascists shared a hatred of liberal and bourgeois Europe that took precedence over their confrontation with communism. Hence, French conservatism was in "fundamental opposition" to fascism. From Barrès to Déat, French national socialists sought to serve the proletariat: George Valois said that fascism was on the side of the proletariat as liberalism had been on the side of the bourgeoisie, and Pierre Drieu La Rochelle defined fascism as a "party of the left."

Sternhell not only takes such rhetoric seriously but also ignores his own evidence when it contradicts his thesis. He himself notes that French fascists never challenged private property or the profit motive, advocated Taylorism instead of class conflict, and were elitists rather than egalitarians. Despite his acknowledgements that George Valois's Faisceau and Pierre Bietry's "yellow" unions were financed by big businessmen like Eugène Mathon and Gaston Japy, that Valois's attempts to recruit communists met with little success, and that Déat's and de Man's neosocialism "permitted capitalism to achieve a legitimacy in the eyes of its own victims," Sternhell repeatedly emphasizes the leftist nature of French fascist thought.

A basic flaw in this approach is his reliance on the writings of fascist intellectuals ("authentic" fascists) rather than on police reports detailing the financial backing and conservative clienteles of actual fascist movements. Sternhell's curious belief that "the nature of political ideology is always clearer in its aspirations than its application" leads him to prefer antibourgeois rhetoric to probourgeois reality: "Of-

ficial fascism as practiced in Italy left much to be desired from the point of view of ideal fascism" (p. 218). Even as historical idealism, this approach falls short, slighting the many probourgeois and procapitalist statements of fascist "socialists" like Valois and Drieu la Rochelle. As for Sternhell's contention that French conservatism was fundamentally opposed ideologically (not just tactically) to French fascism, William Irvine's *French Conservatism in Crisis* (1979) should be read as a much more substantially documented corrective. Sternhell's explanation that French fascists were "finally" thrown back toward the right by their hatred of class politics seems quite lame by comparison.

ROBERT J. SOUCY
Oberlin College

DAVID R. RINGROSE. *Madrid and the Spanish Economy, 1560-1850*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xviii, 405. \$36.50.

Madrid, located on the dry plains of central Castile, rose to prominence after 1561 as the political and social capital of the Spanish Habsburg empire. At the height of its first expansion in the early seventeenth century, its 175,000 inhabitants had to be supplied entirely by overland transport—something without obvious parallel in the premodern world. The implications of supporting Madrid from the weak natural resources of arid Spain are the focus of David R. Ringrose's important book, which blends statistical evidence with a well-defined theoretical matrix.

The first part of the book, comprising about one-half of the text, defines the city's population, occupational structure, and consumption patterns. Around a stable urban core of artisans, nobles, merchants, and bureaucrats, Madrid attracted an envelope of more transient young domestics and unskilled laborers. Most of the city's purchasing power came from tax revenues and landed wealth. Income disparities, already extreme in the late sixteenth century, became more so later, with the poor spending most of their income on staples and the rich favoring luxury items, which were often imported.

The second part of the book analyzes the hinterland that supplied Madrid with food by a combination of incentives and legal obligations. In the areas near the city, government coercion and free-market impulses brought food to Madrid's markets. In those areas farther out, commission agents arranged purchases, establishing lines of supply and drawing local elites into the process of supplying Madrid. The city's increasing need for food began to create a national market system, but it did so without altering the economic, social, and political realities of the

rural Old Regime. In the nineteenth century, a free market took over that was controlled at the local level by landed elites and rich farmers, whose common economic interests eventually led to political collaboration. It was this rural elite that came to control Spanish politics in the nineteenth century after the collapse of the Bourbon monarchy during the Napoleonic wars.

The third part of the book demonstrates how Madrid came to dominate the Spanish economy. In sixteenth-century Castile population growth stimulated agricultural production and manufacturing, and this, in turn, supported a regional network of cities. Limited by the constraints of climate and topography, growth stalled by the end of the sixteenth century, and the network of cities decayed just as Madrid began its spectacular rise as an imperial capital. When the economy entered a recovery at the end of the seventeenth century, Madrid had usurped the functions of most other cities of the interior. The coastal cities of Iberia had their most important contacts outside of Spain, especially in exchanging products of the Spanish empire with Europe. They imported luxuries for Madrid and exported Spanish wool but otherwise had little contact with the interior. Given its nature as a political capitol and the scarce resources of the Castilian countryside, Madrid's rise had a counter-productive effect on the economy, society, and government of imperial Spain.

The mass of statistics and often implicit links with urban and economic theory make the first part of the book sometimes difficult to follow. A nonspecialist might lose the thread of the argument, for Ringrose assumes considerable knowledge of Spanish economic and political history. But overall, the evidence is compelling and clearly presented. The work is based on a large amount of documentary research and on the evidence and conclusions of dozens of modern studies—testimony to recent scholarly production on the Spanish economy. Inevitably, pieces of the model will change as more scholarship appears, but its general shape is likely to hold up. For scholars interested in urban development and European economic history, this book has great importance; for those interested in any aspect of Spanish history since the sixteenth century, it is essential.

CARLA RAHN PHILLIPS
University of Minnesota

JAMES C. BOYAJIAN. *Portuguese Bankers at the Court of Spain, 1626–1650*. New Brunswick, N.J.: Rutgers University Press. 1983. Pp. xiv, 289. \$35.00.

The first half of the seventeenth century saw the height and collapse of Spanish imperialism and the

movement from an economy based on the Mediterranean to one based on the Atlantic. James C. Boyajian's study, which looks at the interaction of these two phenomena from a financial perspective, deserves a wide audience. The starting point is the Castilian bankruptcy of 1627, which saw Portuguese families replace Genoese bankers as the principal financiers of the Spanish Habsburgs. This accomplishment was not a simple replacement; rather, it marked a significant realignment of Castile's policies both domestically and internationally.

Domestically, the involvement of Lisbon bankers in the royal finances of Castile meant a move by the government toward a more open society. As the Portuguese bankers were primarily descendants of recent converts to Christianity, they required a pardon that gave them freedom of movement. Although activities of the Inquisition against New Christians were thus curtailed, the bankers continued to be regarded with suspicion by some sectors of Castilian society. Moreover, because they were identified with the regime of Olivares, they were visible targets for those who opposed his policies. Until the 1640s, though, when the Portuguese rebellion left them in an anomalous position and the fall of Olivares allowed the revival of inquisitorial activity, they were protected and their interests were promoted by the court. The discussion of the symbiotic relationship between the bankers and the regime is notable for its integration of developments in both Iberia and America.

Internationally, the acceptance of the Portuguese as bankers in the 1620s is significant because it indicates an adjustment of royal finances to "the new realities of Atlantic preponderance" (p. 16). The author sees the Portuguese as part of the Atlantic merchant community—indeed, as creators of the Atlantic payments system. This mechanism by which Spain settled its vast international finances meant the replacement of the traditional system dominated by the Genoese, as manifested in the rise of Atlantic exchanges—in Lisbon, London, and eventually Amsterdam—at the expense of Mediterranean ones. In turn, this new system acquired a "momentum and impetus of its own" (p. 99), and in the 1640s, after the military defeat at the Downs and the bankruptcy of 1647, the Dutch and English took over the system created by the Portuguese. The discussion of the development of the Atlantic payments system is both provocative and persuasive, particularly in its emphasis on the priority given to maintaining liquidity in the Antwerp market.

The Portuguese bankers were the last ones "willing and able to assume the extraordinary burdens of Spanish imperialism" (p. 3). The system they created moved beyond their control just as Spanish hegemony came to an end. Boyajian's study, by shedding light on the close relationships between

bankers and the crown, between international finances and international politics, between the death of the Mediterranean and the birth of the Atlantic, enhances our knowledge of the ending of the long sixteenth century.

MICHAEL D. GORDON
Denison University

MERCEDES CABRERA. *La patronal ante la II República: Organizaciones y estrategia, 1931-1936*. Madrid: Siglo Veintiuno. 1983. Pp. 337.

In studying the major bourgeois organizations of the Second Spanish Republic, Mercedes Cabrera has introduced an important new subject into the historiography of the period. Although the political parties and the working-class organizations had been thoroughly (but not exhaustively) studied, a major gap existed before this useful book. The author attempts to close this gap with a well-documented study of the "activity of the . . . industrial, commercial and rural bourgeoisie, organized into pressure groups and owners' associations" (p. 7). She has succeeded in distinguishing among organizations that she accurately claimed were often, but erroneously, portrayed as having a homogeneous approach to the problems faced by the republic.

In addition to providing necessary background and factual information, Cabrera demonstrates that the bourgeois organizations differed sharply in their approach to politics. Readers will hardly be surprised to learn that all agricultural organizations opposed the land reforms of the early 1930s. The author maintains, however, that only the *Agrupación de Propietarios de Fincas Rústicas* had the cohesive social base required to mount an effective resistance to the Agrarian Reform Laws and that it, therefore, spearheaded the opposition (p. 67). That organization, in turn, depended on the broader support of *Confederación Nacional Católico-Agraria* (CNC-A), which appealed to a large number of Catholics with corporatist leanings, especially small landowners who took on themselves the task of fighting for "property, order, family and religion" (p. 311). Less well known are the urban organizations (such as the *Unión Económica* and *Fomento del Trabajo Nacional*) that concentrated on urging greater respect for private initiative. Even when these organizations allied politically with the right, they did not oppose the republic. Rather, they favored a truly free economy, rejecting any state intervention, whether motivated by socialist or corporatist ideology.

The subtitle, "Organizations and Strategy," may be somewhat misleading. On the one hand, the book describes the various organizations and narrates their actions; however, it devotes scant attention to the devising of plans. On the other hand, over half

of the book is devoted to chapters on the depression in Spain, the agrarian question, and industrial development during the republican years. These topics are important to Cabrera's contention that, in the 1930s, Spaniards were convinced that their economic problems were unique, not manifestations of the worldwide depression; moreover, the inclusion of these topics may be necessary in an inexpensive paperback designed to serve a popular as well as scholarly audience. Scholars interested primarily in these aspects of the Spanish economy, however, are advised to consult more specialized works. Of far greater scholarly value are the sections that not only identify but also distinguish among the proprietors' organizations themselves and discussing their supposedly apolitical ideology.

Not since the publication of *Los grupos de presión en la II República* in 1972 has there appeared so extensive a study of these pressure groups. Scholars interested in the political, economic, and social struggles that took place during and just before the Second Spanish Republic should read this book. Although based on broad reading of the primary sources, it is not the definitive scholarly study of these groups; indeed, Cabrera's most lasting accomplishment may be to point to the need for—and give some direction to—more detailed studies of the individual organizations she treats.

WILLIAM J. IRWIN
Bowie State College

NICOLE HAESSENNE-PEREMANS. *Les pauvres et le pouvoir: Assistance et répression au pays de Liège, 1685-1830*. (Anciens Pays et Assemblées d'États, number 81.) Kortrijk-Heule, Belgium: UGA. 1983. Pp. xxiv, 205.

This book by Nicole Haesenne-Peremans intends to provide a case study of social policy, government institutions, and the assumptions informing them by examining the treatment of the Liégeois who lived in poverty under three successive regimes and composed one-fifth of the population.

In many respects, the system changed little between 1685 and 1830. Liège's Old Regime measures of poor relief were limited in scale, underfunded, and ineffective, and, although the principality participated in the "Great Confinement" sweeping across Europe, this failed to reduce either poverty or mendicancy. By 1770 the existing structure was under siege by philanthropists imbued with notions of individual liberty, manufacturers angered by competition from low-wage pauper labor, and taxpayers demanding an end to expensive building projects. Simultaneously, enlightened ideas, a more positive conception of the state's role in welfare, and fear of the impoverished engendered numerous reform proposals. Despite this groundswell favoring

change and its own grandiose projects for aiding the poor, the new regime that took power in 1794 quickly shelved all notions of reform in order to defend the revolution. Instead of trying bravely to uproot poverty, the new authorities, like their predecessors, sought to prevent disorder and thus failed to alleviate either chronic distress or the misery due to revolutionary convulsions. The Dutch government that ruled between 1814 and 1830 largely perpetuated the traditional measures, with equally disappointing results.

Even if authorities proved powerless to limit poverty and if most reform proposals were anachronistic, Haesenne-Peremans does find a few initiatives directed at eradicating the causes of poverty rather than simply managing its effects. In 1773 a councilor to the prince-bishop proposed generating relief funds by suppressing religious houses, targeting aid to the unemployed, and providing jobs, while early nineteenth-century policymakers settled beggar families on uncultivated land in the Campine and encouraged institutions designed to teach orderly behavior, thrift, and foresight. Although the author admits that few people actually benefited from these plans, she argues that they indicate the beginning of a crucial change in attitude from assistance to insurance (*prévoyance*) and thus represented the forerunners of contemporary social security.

Although the book provides an interesting look at elite attitudes toward poverty and the poor and their implementation, its potential usefulness to historians of poverty and welfare policy is not realized. The author neglects the ways in which the socioeconomic transformations that Liège was undergoing, the composition of the successive regimes, and popular attitudes shaped policy; indeed, the book remains largely an internal history of ideas and institutions. Despite the adoption of a Foucaultian framework and a few comparisons with conditions elsewhere, the scholarly literature is neither systematically employed nor critically addressed. Haesenne-Peremans adduces little evidence to support her hypothesis that welfare policies were dictated most often by fear of the poor; nor does she consider the possibility, forcefully advanced in Hugo Soly and Catharina Lis's *Poverty and Capitalism in Pre-Industrial Europe* (1979), that labor market regulation was the authorities' primary concern.

The book does have the merit of showing the slow pace of change in social policy and practices, but readers trying to understand the dynamics of continuity and discontinuity in preindustrial welfare systems still need to consult Soly and Lis or Jean-Pierre Gutton's *La société et les pauvres en Europe* (1971).

ROBERT S. DUPLESSIS
Swarthmore College

JOHN W. ROONEY, JR. *Revolt in the Netherlands: Brussels, 1830*. Lawrence, Kans.: Coronado. 1982. Pp. vii, 250. \$12.50.

Although more than a century and a half has passed since the Belgians rose in revolt against Dutch rule, historians have shown relatively little interest in the upheaval. Consequently, the events of August 25–October 27, 1830 have been only partially understood at best and, at worst, utterly misunderstood. We can only applaud the appearance of John W. Rooney, Jr.'s *Revolt in the Netherlands: Brussels, 1830*, which goes far toward setting the record straight. The basic narrative is grounded in extensive archival holdings as well as in relevant published primary and secondary literature. Well written and convincing, the work calls for revision in our thinking on a number of points. A brief review, however, can barely indicate the importance of this contribution to Belgian historiography.

Rooney emphasizes a point made by some other writers but not widely enough accepted—the Belgian revolt was no “spin-off” from the July Days in Paris. A bona fide revolutionary situation existed in the Belgian provinces, and the outbreak of violence had nothing to do with the upheaval that had so recently cost Charles X his throne.

The author gives a clear statement of conditions leading to the revolution and then discusses its progress, carefully describing differences of opinion among and within various groups. The presentation of King William I is sympathetic. Although noting errors of judgment by William, Rooney finds a great deal to say that is favorable to the Dutch monarch. The same is true for Prince William and Prince Frederick. All three he sees as tragic figures caught in circumstances they were unable to control.

Rooney did not find religious and linguistic differences to be the critical factors causing the revolution. More important were policies that had alienated (1) the Flemish clergy, (2) the legal profession in the Belgian provinces, and (3) the liberal press in the southern provinces. These three groups provided leadership for the revolution, and many individuals in these groups were prominent in negotiations at several levels and tended to lend their ideas to the constitutional arrangement that emerged. Hence, the world has judged the Belgian revolt to be largely a liberal revolution, “obviously” related to the one in Paris.

Rooney's study makes its greatest contribution, however, by demonstrating convincingly that the motive force of the revolt came from the lowest classes in Brussels. Using data compiled from hospitals and aid stations on the killed and wounded, the author makes clear the extensive participation of the working classes. Indeed, at the height of the revolt, bourgeois leaders fled the city, and its de-

fense was left to an untrained, leaderless rabble that fought intermittently. Dutch errors of judgment prevented the fall of the city, and, when its safety seemed assured, the liberals returned and assumed leadership. They became spokesmen for the revolution, and history has largely forgotten the workers.

BRISON D. GOOCH
Texas A&M University

CARL-JOHAN GADD. *Järn och potatis: Jordbruk, teknik och social omvandling i Skaraborgs län, 1750–1860* [Iron and Potatoes: Agriculture, Technique, and Social Change in the County of Skaraborg, 1750–1860]. (Meddelanden Från Ekonomisk-Historiska Institutionen vid Göteborgs Universitet, number 53.) Gothenburg, Sweden: Institute of Economic History, Gothenburg University. 1983. Pp. 372.

It is now acknowledged that the remarkable growth of incomes in late nineteenth-century Sweden had its origins in the far-reaching changes and successful response in the agricultural sector that began a century earlier. The first clear evidence of this was the development of the export trade in oats from the southwest after 1850. Yet how was it that, when the population began to grow rapidly, a food surplus could be produced from the cold, wet plains and stony forest clearings of Skaraborg by a society that in the mid-eighteenth century had been living at subsistence level? The national, juridical, and institutional changes imposed on agriculture provide part of the answer, but the rest has to be found in the detailed study of the response at a local level.

Building on the work of Lars Herlitz, Carl-Johan Gadd has studied (most interestingly by using samples of probate inventories for peasant households over the period 1748–1859) five pastorates on the clay plains and one in the forest, together with two additional areas that are used as a control for the capital stock figures. His work shows that the hypothesis that in all such transitions the necessary increase in arable output must first come from an increase in labor productivity (essentially from longer hours of work) need not apply.

Sweden was more fortunate. During the period between 1770 and 1780 the disruption of the ecological balance through the extension of arable land led to more severe pressures on food supply and incomes. But the increased use of iron for implements and wagons, perhaps stimulated by a fall in prices during the French wars, coupled with the increase in potato production in the forest areas, generated a sufficient increase in overall productivity to eliminate the need for an increase in labor productivity. On the plains it was possible, with no increase in labor inputs per unit of yield, to double

arable output. At the same time, improvements in communications permitted a fall in the livestock population of about the same proportions as that required by the switch to arable farming to feed the growing population. Only after 1770, as the new rotations were introduced, was any increase in labor productivity imposed by agricultural change.

Gadd's work brings to light much valuable detailed information. Skaraborg peasant households were, in fact, already relatively well-off on a European scale in the mid-eighteenth century; on the plains they owned, on average, four draft animals, six cattle, a plough, and wagons. Iron ploughs came into use rapidly in the 1790s and were almost universal by 1820. The use of wagons with iron axles spread at the same time. Of course, this book says little about the condition of cottagers and the landless, who constituted the major part of the increase in households. Nevertheless, it is most systematically done and makes a valuable, intelligent contribution to the study of an important problem.

ALAN S. MILWARD
European University Institute
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BIRGIT LØGSTRUP. *Jorddrot og offentlig administrator: Godsejerstyret inden for skatte- og udskrivningsvæsenet i det 18. århundrede* [Landowner and Public Administrator: Taxation and Conscription on Eighteenth-Century Manors]. Summary in German. (Administrationshistoriske Studier, number 7.) Copenhagen: Rigsarkivet. 1983. Pp. 479.

East Elbian absolutism rested by the eighteenth century on a compromise between autocrats and manorial lords, who fulfilled basic public functions in return for fiscal immunities and an essentially free hand on their own estates. That this was the case in Denmark—unlike in the rest of Scandinavia—has long been recognized. Now Birgit Løgstrup has provided an in-depth study of how this "patrimonial" system (in Max Weber's terminology) actually functioned.

Following a useful survey of the seigneurial regime as an integrated system, Løgstrup takes up her specific topic: the role of the Danish landlords in rural tax collection and military conscription. She explains in great detail and with numerous examples the organization, personnel, operating procedures, record keeping, adjudication, and not least the practical problems involved at the grassroots level. Aptly captioned contemporary illustrations reinforce the text. The disappearance or incompleteness of many private estate archives has complicated Løgstrup's task. She uses the well-documented Løvenborg manor on Sjælland as her prime

example, supplementing this with available material from other estates.

What emerges is a balance not only between crown and landlord, but also between the latter and his tenantry. As he was personally accountable for his tenants' land tax and quota of conscripts and yet forbidden to add their farms to the demesne, the landlord was often compelled to make accommodations to keep his tenancies filled. He could meanwhile use his public functions to bring pressures to bear on the manor's peasants in his own private interests.

Løgstrup's discussion of the emergence and breakdown of the patrimonial system—both of which occurred largely during the eighteenth century—is of particular interest and surely of wider relevance outside of Denmark. She reveals how a relatively bureaucratized state administration in the early seventeenth century was replaced, after the establishment of absolutism in 1660, by a financially simplified, largely debureaucratized system based on a proprietorial class (now no longer exclusively noble) that on losing political power reinforced its seigniorial prerogatives. She shows how, from the early seventeenth century, landlords voluntarily assumed public responsibilities to preserve manorial immunities and authority, as, for instance, in connection with the adscription law (*stavnsbånd*) of 1733–88 and the poll tax of 1762–1812. Simultaneously, the widespread restructuring from scattered tenancies to compact holdings suited to demesne farming with tenant *corvées* made this arrangement advantageous to both crown and proprietor.

Hans Jensen maintained in 1936 that the rise of an effective state bureaucracy ultimately broke the proprietorial regime in Denmark, a thesis that has been much debated since. Løgstrup stresses instead the internal breakdown of the patrimonial system due to the mounting burden of manorial responsibilities and the increasing number of peasant freehold purchases after 1760. As a result, landlords lost incentives and desire to administer. With the reforms of the 1780s and 1790s a new and more sophisticated bureaucratization largely removed them as intermediaries between the crown and the mass of its subjects.

Løgstrup has made an important contribution to our understanding of the "landlord and public administrator" (the title of her book) and, in so doing, has illuminated many central questions of eighteenth-century Danish and European history.

H. ARNOLD BARTON
Southern Illinois University,
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HANS KRYGER LARSEN. *Merkantilismen i dansk historievidenskab, 1890–1940: En historiografisk undersøgelse af*

Albert Olsens forfatterskab og hans placering i forskersamfundet [Mercantilism in Danish Historiography, 1890–1940: A Study of the Danish Historian Albert Olsen's Work and Position in the Academic Community]. (Københavns Universitet, Institut for Økonomisk Historie, number 19.) Copenhagen: Akademisk. 1983. Pp. 152. 134.20 KR.

In the 1960s student protesters and outspoken young scholars breached the socially prestigious, financially secure, and intellectually stultifying hot-houses in which Danish university history professors had worked since the professionalization of the discipline in the 1890s. Many of the rebellious historians were politically engaged and irked by their antagonists' pretension to classlessness and ideological objectivity. Consequently, a considerable proportion of the new generation's work since the mid-1960s has been revisionist historiography based on the thesis that the work of the established professoriate and their predecessors was influenced by class origins and subjective interests. Scores of review essays, articles, and books have exhumed the dead and disembowled the living. Hans Kryger Larsen is one combatant who grew tired of this ghoulish enterprise and decided to tackle more significant problems, but a promising monograph on mercantilism is marred by his inability to break completely with his own past.

In a short but very dense and thought-provoking book, Larsen analyzes changing conceptions of mercantilism among Danish university professors from 1890 to 1940, focuses on the work of Albert Olsen (1890–1949), and compares the Danish scene to developments in Germany. Larsen argues that Olsen's experiences as a journalist for *Socialdemokraten* changed his intellectual assumptions and scholarly interpretation of mercantilism. As a student of the professorial rebel, Erik Arup, Olsen had combined classical liberalism with economic determinism. The liberal interpretation depicted mercantilism as a progressive state policy that sundered the medieval universe and initiated modern economic development. Although Olsen's liberal interpretation retained a focus on the individual entrepreneur, he assumed that fundamental economic forces established a limiting framework for mercantile politics. In the 1920s, however, Olsen began to doubt the efficacy of laissez-faire liberalism. His ingrained evolutionary perspective prevented him from joining the antiliberals of the right or the left, who argued that the capitalistic phase of mercantilism was an aberration requiring the return to a state-regulated economy. Instead, Olsen adopted a social-liberal position that altered his interpretation of mercantilism. The individual entrepreneur was replaced by an enlightened state bureaucracy as the primary vehicle for stimulating economic growth

within a capitalistic system. For Olsen, mercantilism had become a necessary transition between the medieval world and modernity; the final goal of the historical process had become the social-liberal conception of an equal partnership between capital and worker, achieved by an intelligent state response to economic forces rather than class struggle.

One of Larsen's major problems is to overcome Olsen's refusal to acknowledge subjective influences on his changing views of mercantilism. Olsen's claims to scientific objectivity and rigorous positivism cause Larsen to revive the outdated debates about the innate subjectivity of historical investigation. We learn that Larsen has forsaken the Kuhnian paradigms of his colleagues at the University of Odense for Habermas's concept of *ideologie*, but this concept can never make up for the paucity of solid evidence about Olsen's motivations for making the shift from classical liberal to social liberal. In fact, Larsen's own suppositions are imaginative, reasonable, and eminently more persuasive than the theoretical jargon that comprises chapter 1 and is sprinkled throughout the remainder of the book. It is a distressing commentary on Scandinavian historiography when a person of Larsen's analytical abilities wastes part of an excellent book on disputes with a small clique of Danish historians. It is also ironic that this same clique often removes itself as much from the lay readership and their own international colleagues as the entrenched professoriate they pilloried in the 1960s.

PETER VINTEN-JOHANSEN
Michigan State University

ROAR SKOVMAND. *Samspillet mellem Nordens Folkehøjskoler indtil Anden Verdenskrig* [Interaction among Scandinavia's Folk High Schools until the Second World War]. (Skrifter Udgivet af Jysk Selskab for Historie, number 41.) Aarhus: Universitetsforlaget. 1983. Pp. 183.

The Danish *folkehøjskole* was a unique educational phenomenon. It started in the mid-nineteenth century (the first school was established in 1844), partially as an outgrowth of Danish nationalism, itself a response to growing German nationalism. By 1901, 30 percent of any given rural age cohort had attended a *folkehøjskole*. The schools provided six-month, live-in, adult-education courses, which appealed especially to rural youths who had dropped out of school after the compulsory seven years of education. The schools were extremely important in the development of the co-op movement, the Agrarian party, and the concurrent modernization of Danish agriculture.

Were they exportable? The schools were singularly unsuccessful in the United States, which had a

much longer period of compulsory education and whose liberal arts colleges served many of the same collegial functions. But in Norway, Sweden, and Finland there were many similarities to Denmark. These countries were also basically agricultural societies, where the only form of additional education was the formal professional university and where there were growing nationalist movements. And here, indeed, the *folkehøjskoler* did take hold.

Roar Skovmand's book, *Samspillet mellem Nordens Folkehøjskoler*, however, is not a discussion of the social and cultural bases of the *folkehøjskole* movement. Rather, it is the account of the relationship between the principals who started the schools. This is the story of a small group of men, and their wives, who all either visited or attended Danish schools and then went home to establish similar ones. They kept contact with each other by writing letters ("reports to headquarters," as one principal put it) and holding periodic conferences.

Skovmand has apparently limited his sources to these letters and to printed conference programs and reports (although he does include footnotes, there is no bibliography). He comments at one point that the articles in *Højskolebladet*, with which he documents his accounts, were based on "steno-graphic notes" of the 1926 meeting (p. 151), leading one to wonder on what the earlier reports were based. Skovmand is a reporter; there is virtually no analysis in his book. He does not discuss the success or failure of the different schools, nor how they changed or differed from place to place. He does not even paint portraits of his main characters, the principals. Students are surprisingly absent from his pages.

Skovmand's book is only for those already familiar with the *folkehøjskoler*. He assumes that his readers understand the movement, its ideals, and debates and that they are, for example, cognizant of the intricacies of the discussion on Grundtvigianism. He also assumes a basic knowledge of the major events in nineteenth-century Scandinavian history, because he gives virtually no background material. He is addressing a very limited audience.

CAROL GOLD
University of Alaska

AUVO KOSTIAINEN. *Santeri Nuorteva: Kansainvälinen suomalainen* [Santeri Nuorteva: An International Finn]. Summary in English. (Historiallisia Tutkimuksia, number 120.) Helsinki: Suomen Historiallinen Seura. 1983. Pp. 224.

This short biography's subject is Santeri Nuorteva (Alexander Nyberg [1881–1929]), a relatively obscure but apparently important Finnish radical and an elusive and mysterious international conspirator.

Having begun as a school teacher, Nuorteva later became a journalist for radical newspapers in Forsa, Turku, and Tampere. He joined the Socialist party and was elected three times to the country's newly established diet. In 1909 his political views brought him a six-month prison term. Unrepentant, he soon returned to his old ways; new charges were levied, and in late 1911 Nuorteva fled to the United States.

In the New World Nuorteva worked on several immigrant papers and became influential in the Finnish-American socialist movement. After the Bolsheviks seized power in Russia in 1917, Nuorteva was asked in the spring of 1918 to set up a Finnish Information Bureau in New York to promote the objectives of Finland's Reds. It shortly became the Soviet Information Bureau, and Nuorteva was joined by other publicists. He became totally committed to the communist cause and thereby became a target of U.S. intelligence investigations. The "red diplomat" left the United States in June 1920.

Nuorteva returned to Europe by way of Canada and Great Britain, where he was arrested as a "dangerous radical" and deported. He reached the USSR in August 1920. He was employed on two different occasions in the Commissariat for Foreign Affairs as head of its Anglo-American section. He served a longer period of time and with greater distinction in Karelia, where he became chairman of the autonomous republic's central committee. For reasons not yet fully explained, Nuorteva was imprisoned for nearly a year in 1921–22.

Nuorteva had many talents. He had command of, it was said, fifteen languages, including those most important for his international activity—Russian (his mother was Russian) and English. He was a remarkably persuasive writer and speaker and had an ingratiating personality. He was boundlessly energetic and brimming with ideas, not all of which, scoffed his critics, were very practical.

Auvo Kostiainen's impressive bibliography, overabundant footnotes, and wide-ranging research notwithstanding, this work is plagued unavoidably at every turn with perplexing, unanswerable questions. What were Nuorteva's connections with the Comintern? How did he actually operate? Who were his contacts? How, precisely, did he earn his sinister reputation? As Kostiainen constantly reminds his readers, the required sources are not available or are inadequate. Soviet archives remain closed. Existing studies on Nuorteva are generally short, inconsistent, and impressionistic. Nuorteva's own biographical contributions are not very revealing. Apparently, there are no central Nuorteva archives as such. Kostiainen offers an interesting and good beginning, but it is not and cannot be at this juncture a full and definitive interpretation of Nuorteva. It may be that a full-scale biography will

never be possible, even though Nuorteva may merit greater recognition in the annals of history.

JOHN I. KOLEHMAINEN
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ALPO RUSI. *Lehdistösensuuri jatkosodassa: Sanan valvonta sodankäynnin välineenä, 1941–1944* [Press Censorship during the Continuation War: Implementation of Control of Expression during the War, 1941–44]. (Historiallisia Tutkimuksia, number 118.) Helsinki: Suomen Historiallinen Seura. 1982. Pp. 394.

During the so-called Continuation War (1941–44), Finland's political and military leaders were called on to perform a delicate balancing act between the interests of the Finnish people and state and the demands of a powerful cobelligerent, Nazi Germany. As Alpo Rusi shows in his doctoral thesis, the press censorship, known under the acronym of VTL, played a crucial role in mediating the thoughts and actions of the leadership. In so doing, VTL also appears to have shared the confusion and lack of unambiguous clarity that existed among the "inner circle" over war aims and Finland's relationship with Germany. The theme of Finland's "separate war" was rather blurred in the initial euphoria sparked by German victories on the eastern front, and VTL also tended to give credence to Mannerheim's famous Order of the Day of July 10, 1941, which envisaged the acquisition of East Karelia. Rusi observes that the way in which the declaration was made, rather than its contents, is what aroused the politicians: the lack of clearly defined war aims on the part of the government meant that the "Greater Finland" vision of the nationalist Finnish-speaking element was sustained even when the war began to turn decisively in the favor of the Grand Alliance. "It is better for the nation to be united on the wrong side than divided on the right side": Urho Kekkonen's wartime adage served as a kind of motto for VTL, Rusi argues, although this begs the question of how that unity was to be achieved. As earlier studies by Touko Perko and Esko Salminen have shown, the initial general enthusiasm for a war to rectify what was perceived as an unjust Peace of Moscow began to fade in 1942. As doubts began to surface, censorship became more active. Fear of German retribution undoubtedly persuaded the authorities to curb speculation on the desirability of a separate peace, although they also permitted the press to use Allied news sources from the autumn of 1942—so much so that General Erfurth complained in March 1944 that a German soldier reading a Finnish newspaper might think he was in an enemy country. In fact, the Finnish press was a good deal more restrained than the sharp-

eyed German observers imagined, both in terms of the censorship, which became much tighter in the summer crisis of 1944, and in terms of editorial policy. VTL rarely had to resort to heavy-handed sanctions.

Rusi's book, in spite of the poor quality of the print and numerous printing errors, offers a stimulating account of the dilemma in which the Finnish war leadership found itself as the hopes of 1941 faded. It is evident that Mannerheim exercised considerably more influence over decision making than his formal post of commander-in-chief provided for and that this influence spilled over into the molding of public opinion. In the summer of 1944 the immediate necessity of obtaining military support from Germany to stem the Red Army's assault overshadowed the need to prepare the people for a separate peace; the censor's role was thus more preventive than instructive, and, as a result, the press was ill prepared for the shock of peace negotiations. In the circumstances, however, it is hard to see what alternative line could have been followed. The Finnish press was also blinkered by the hopes and fears of its editors and readers, although never sufficiently to block out an awareness of the ultimate necessity of coming to terms with a situation that was determined by the major protagonists in the war. In spite of the censor—or perhaps because of the censor—this necessity was acknowledged by leading journalists such as Reinhold Svento and Urho Kekkonen ("Pekka Peitsi"), and in this sense the ground was prepared for the new foreign policy of the postwar era, a point not missed by the author.

DAVID KIRBY
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CARSTEN KÜTHER. *Menschen auf der Strasse: Vagierende Unterschichten in Bayern, Franken und Schwaben in der zweiten Hälfte des 18. Jahrhunderts*. (Kritische Studien zur Geschichtswissenschaft, number 56.) Göttingen: Vandenhoeck und Ruprecht. 1983. Pp. 173. DM 29.80.

There is nothing new any more about doing history "from the bottom up." But sometimes it is hard to get to the very bottom of things. Vastly more records have survived concerning stable, continuous institutions—families, households, occupational groups, communities—than are available for studying the nethermost members of past societies—the rootless beggars, vagrants, and criminals who wandered from place to place in search of money and bread. Often these groups can be examined only by using special source materials that document their limited forms of interaction with the more established sectors of society—particularly, though not

exclusively, by using records of criminal investigations and trials.

Carsten Küther is an expert on records of this sort, having used them extensively in his study of organized robber bands in eighteenth-century Germany, *Räuber und Gauner in Deutschland* (1976). The present study is narrower in geographical and chronological focus, confined as it is to southeastern Germany between 1750 and 1800. But it is broader in theme, for here the author treats the entire range of "Menschen auf der Strasse"—people on the open road. Once again criminal records are his major source, for virtually all vagrants were regarded by the authorities as criminals. Simply to travel from one place to another without the papers and passes normally required to document the purpose of every journey constituted, in many cases, a criminal act. The authorities of Bavaria and the smaller states that surrounded it would repeatedly apprehend such undocumented individuals and, usually after giving them a sound whipping, transport them to the nearest border with orders to return "home"—as if a vagrant always had one. When suspected of specific criminal acts, of course, the vagrants faced even harsher penalties.

It is a bit difficult to follow the chain of calculations by which the author arrives at his estimate that vagrants made up one-tenth of the population of Bavaria in the late eighteenth century. But one can only applaud Küther's sensitively differentiated description of the various groups that made up the vagrant population. Some vagrants, he shows, were only temporarily rootless; by maintaining links to their home town or former occupation, they could eventually hope to be reintegrated into settled society. Other vagrants, by contrast, were so alienated from established society as to make any eventual reintegration impossible. Former soldiers, for example, made up a large portion of the permanent vagrants; deserters could never return home, and even properly discharged soldiers were often unwelcome in their original communities. As one might expect, women represented a minority among the vagrants, but once a woman had become a vagrant her chances of ever being reintegrated into settled society were particularly small.

Küther's refreshingly short text is supplemented by an absorbing selection of documents pertaining to the apprehension and punishment of vagrants in the era under study. A number of these documents record the interrogation of suspected vagrants, along with their verbatim responses. In the vagrants' nervous, evasive replies to the unremitting queries of the authorities, we are given a rare opportunity to hear voices from the very bottom of the social ladder. These are the words of people who must have regarded even the overtaxed peasants, oppressed laborers, and exploited servants of the an-

cien régime not with scorn or pity, but with sheer envy for enjoying a way of life still far, far better than their own.

CHRISTOPHER R. FRIEDRICH
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T. C. W. BLANNING. *The French Revolution in Germany: Occupation and Resistance in the Rhineland, 1792–1802*. New York: Clarendon Press of Oxford University Press. 1983. Pp. vi, 353. \$45.00.

T. C. W. Blanning criticizes Jacques Godechot's *The Counter Revolution* (1971) because it "is distorted by an unconcealed hostility to the subject" (p. 317 n). This work suffers from the same deficiency. The contribution of Blanning's considerable research is lessened by his pronounced dislike of the French Revolution and France's attempt to export it. The book is a polemic aimed at two historiographical currents. Against "a wholly unrepentant" (p. 12 n) R. R. Palmer and supporters of the idea of an age of democratic revolution, Blanning asserts that, except in France itself, the 1790s was an "age of counter-revolution" (p. 336). Against Heinrich Scheel and others who try to demonstrate the existence of a significant Rhenish Jacobin tradition, Blanning argues that the German supporters of the French Revolution were but a miniscule, isolated minority in the midst of a population that loathed the French as corrupt, cynical conquerors who ruthlessly exploited the Rhineland for all it was worth.

Blanning sets out to answer the questions: "What did the occupation [of the Rhineland by the French] mean, in concrete, tangible terms, for the occupied? Why and how did they resist the political, economic, and religious policies of the new regime imposed by the French?" (p. 15). He has read innumerable local chronicles, diaries, pamphlets, and newspapers, worked in the major municipal and state archives, and made thorough use of Joseph Hansen's monumental document collection. From these varied sources Blanning is able to show better than anyone before that to maintain a huge army under conditions of almost continuous war France had to exploit the conquered German lands without letup. Naturally, most Rhinelanders protested and often resisted—through noncooperation, illegal activities such as smuggling, and an occasional riot. Of particular value here are the descriptions of military requisitions, of the damage done to the local economy, and of the Rhinelanders' struggle to preserve the trappings of their Catholic faith.

The treatment of political developments, however, is far less satisfactory. Blanning rightly exposes the weakness of Rhenish Jacobinism, but he misuses present-day nationalistic concepts when he declares that "if a state as repellent as National Socialist

Germany could find quislings to do its evil work, it is not surprising that revolutionary France should have attracted collaborators. This co-operation between conqueror and conquered does not merit, however, the description 'The age of National Socialist Revolution' or 'The age of the Democratic Revolution' respectively" (p. 13). In fact, once it became clear that the Old Regime was gone for good, many respected notables in the Rhenish towns worked closely and continuously with the French without being condemned as collaborators either by their fellow citizens or by the German regimes that replaced the French after Napoleon's fall.

Blanning's desire to prove mutual French-German antipathy leads him to make exaggerated and incorrect statements. "At no time, not even under the more settled conditions after 1802, did a German attain high office under the French" (pp. 273–74). Does it not count as high office when Rhinelanders became mayors, subprefects, judges, even members of the French Senate and Corps Législatif? "Every order from the authorities—to erect a liberty tree, to wear a tricolour cockade, to attend a republican ceremony, or whatever—was countered by stubborn non-co-operation" (p. 304). Some orders were of course resisted, but by no means every one. The Rhinelanders are called "an ungovernable people," meaning that "they could be controlled and they could be coerced, but they could not be *governed*, in the sense of orderly administration by established procedures within a generally accepted framework" (p. 314). But from 1800 on they were governed quite peacefully and effectively, and after 1815 Rhinelanders fought to retain those very laws and institutions that France had imposed on them. Unfortunately, Blanning's hostility toward the revolution and his sympathy for the counterrevolution leads him to distort the political transformations that had taken place.

JEFFREY M. DIEFENDORF
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WALTER DEMEL. *Der bayerische Staatsabsolutismus, 1806/08–1817: Staats- und gesellschaftspolitische Motivationen und Hintergründe der Reformära in der ersten Phase des Königreichs Bayern*. (Schriftenreihe zur Bayerischen Landesgeschichte, number 76.) Munich: C. H. Beck. 1983. Pp. xxxv, 595.

Walter Demel's study, originally a 1981 dissertation, illustrates both the vitality of regional history in Germany and the growing scholarly interest in German reforms during the period of Napoleonic domination. Demel was stimulated to write this monograph by his mentor, Eberhard Weis, whose biography of Maximilian Montgelas (the founder of the modern state of Bavaria as its first minister from

1799 to 1817) appeared in 1971. Weis, along with others like Elizabeth Fehrenbach, Werner Schubert, and Bernd Wunder, has helped bring the study of south German reform movements of the early nineteenth century into the mainstream of current German historiography.

Demel notes Montgelas's central role in Bavarian affairs but focuses his work on approximately thirty high civil servants and the many reforms they proposed, attempted, and sometimes implemented from 1806–08, the period after Bavaria gained full sovereignty, to Montgelas's dismissal in 1817. The king, Maximilian Josef, took little initiative in the reforms that in effect established the Bavarian state as the absolute political authority within the aggrandized territory of the new kingdom.

The richness of issues Demel covers can only be hinted at here. Part 1 analyzes the political background, social and economic conditions, and the general goals of the bureaucratic reformers. Part 2 roots the financial crisis that led to reforms in the exorbitant military expenditures and soaring debt of the state. The secularization of ecclesiastical properties and the absorption of many princely holdings did not improve state finances, although these measures increased the number of potential taxpayers and soldiers. Yet the administrative incorporation into Bavaria of these lands, along with other new territories lacking a tradition of estates, made more radical reforms for the entire kingdom possible.

The establishment of a centralized administration based on a reformed, tenured, and modernized civil service and the introduction of equal taxation and other tax reforms laid the groundwork for the completion of state absolutism (part 3)—the ending of municipal privileges, the attempt to incorporate the quasi-state functions of the nobility into general administration, and the reregulating of church-state relations.

The longest section, part 4, deals with freedom and property and is significantly subtitled "The State's Protective Functions as Grounds for Justifying the Expansion of State Power." Liberals gained some important reforms: the principled recognition of the individual's sphere of freedom, the guarantee of freedom of conscience and religion, security of capitalist property (*bürgerliche Eigentum*), and some liberalization of the guild system. Other reforms—freedom of the press, free trade, the complete disappearance of all feudal remnants—were not so thoroughly liberal. Demel argues that Montgelas cooled the ardor of some reformers, fearing that the social and fiscal consequences of some changes might weaken the state's authority. In his summary Demel indicates that the aim of reform was to strengthen the state. With the introduction of a constitution and a diet in 1818 a new phase, growing out of the old, came into being.

Demel's full title accurately states the contents of his study—we read about bureaucrats, their ideas, and their reforms. Demel has studied these with extraordinary thoroughness and has examined almost every issue that engaged their efforts. What this study is not—a point often lamented for this period—is a contribution to what ordinary Bavarians thought about all of this and to what effect society, aside from the nobility, had on the state and its officials. Also, although Demel rightly points out that the reform impulse came from within Bavaria (not from French pressure alone) and sometimes preceded similar events in other German states, he does not systematically place Bavarian developments within the general German reform period. Studies such as his, however, will ease that task.

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BARBARA VOGEL. *Allgemeine Gewerbefreiheit: Die Reformpolitik des preussischen Staatskanzlers Hardenberg, 1810–1820*. (Kritische Studien zur Geschichtswissenschaft, number 57.) Göttingen: Vandenhoeck und Ruprecht. 1983. Pp. 340. DM 68.

Anyone picking up a book with "General Freedom of Industry" in the title and "The Reform Policy of Prussian State Chancellor Hardenberg" in the subtitle might expect to find a highly specialized monographic study on the repeal of guild regulations of manufacture in the Hohenzollern monarchy during the early years of the nineteenth century. But such an assumption, though certainly logical, would be mistaken. For what Barbara Vogel has tried to do is examine the policy of industrial liberalization as part of a far-reaching process of "modernization" that Prussia underwent during the period of the French hegemony. For Vogel, the most important figure of the reform era was not Stein, with his corporative sympathies and traditionalist loyalties, but Hardenberg, the enlightened bureaucrat who sought to combine the introduction of free enterprise with the maintenance of centralized authority. She argues that for Hardenberg "freedom of industry" meant not only the elimination of restrictions on competition in handicraft and manufacture but also, as expressed in his Riga Memoir of 1807, "the establishment of the freest possible use of the energies of subjects from all classes." In this sense, the promulgation of "freedom of industry" should be seen as part of that peculiar mixture of economic liberalism and political authoritarianism that characterized the history of Germany for more than a hundred years.

In developing this theme, the author employs a sociological analysis of the Prussian state and bureaucracy at the end of the Napoleonic era. Like

many German scholars of the younger generation, she relies heavily on the insights and perceptions of the social sciences. She talks about theories of modernization, the nature of reform as opposed to revolution, the essence of industrialization, the concept of a traditional or premodern society, and the "take-off into sustained economic growth." There may be some readers who will become a little impatient with all of the conceptualizing and theorizing, with the explication, exegesis, analysis, and scholarly apparatus.

But once Vogel finishes with her theorems and definitions, once she begins to deal with the concrete policy of the government under Hardenberg, the narrative becomes illuminating and persuasive. She describes the role of the bureaucracy in an autocratic state like Prussia, examining its social and educational background, its method of selection and training, and its system of remuneration. She then turns to the way in which it exercised its authority, emphasizing the conflict of personalities, the importance of the chancellor's office, and the "bureaucratic absolutism" and "quasi-constitution" of the monarchy. Finally, there is a discussion of the nature of "public opinion" in a state in which popular interest in government affairs was generally discouraged. In the light of this institutional analysis, the author examines in detail the legislation of 1810–11 establishing "freedom of industry" in Prussia and describes its economic and social consequences. Her book is of interest not only for what it has to say about the introduction of free enterprise, but also for its insight into the interaction of state, society, and economy in Central Europe during the nineteenth century.

THEODORE S. HAMEROW
University of Wisconsin

E. I. KOURI. *Der deutsche Protestantismus und die soziale Frage, 1870–1919: Zur Sozialpolitik im Bildungsbürgertum*. (Arbeiten zur Kirchengeschichte, number 55.) New York: Walter de Gruyter. 1984. Pp. x, 256. \$49.10.

This book joins a substantial list of recent works on the role of the churches in Germany's social and political development. Its title calls to mind the classic study by William O. Shanahan, *German Protestants Face the Social Question* (1954). Shanahan dealt only with the period before 1871, and those who regret the absence of a sequel will doubtless welcome E. I. Kouri's concise and generously documented survey of the imperial era. Although the author does not explain what led a specialist in Elizabethan diplomacy to explore such apparently unfamiliar scholarly terrain, his choice of a topic clearly underscores the importance accorded to the

Second Reich in current German historiography. It likewise bears witness to historians' renewed inclination to treat church history as an integral part of social history rather than as an encapsulated sub-discipline of interest chiefly to its own sectarian practitioners.

As the subtitle suggests, Kouri sees churchmen as typical representatives of the educated German middle classes. He outlines the social and cultural situation of these groups in an introductory chapter that constitutes a veritable compendium of recent research on industrialization and social change after unification. The remainder of the study concentrates particularly on the Wilhelmine period, with a somewhat perfunctory discussion of the Bismarck years and an epilogue sketching the dissolution of the Protestant synthesis after 1918. Drawing heavily on the work of Günter Brakelmann and Klaus Erich Pollmann, among others, Kouri presents a fairly predictable chronicle of failures and stillborn initiatives, of ecclesiastical *Staatsfrömmigkeit*, class isolation, and lack of clerical nerve. During the early 1890s, a breakthrough of sorts briefly appeared possible in the reformism of churchmen who, like Friedrich Naumann, sought to combine Protestant ethics with empirical social and economic analysis in search of rapprochement with the working classes. This "socialism of the pastors" quickly foundered, however, because of opposition from church leaders, the imperviousness of proletarian culture to most middle-class appeals, and deep divisions over confessional and tactical positions within the reformist front itself. The dissipation of Protestant social energy, Kouri insists, was all but complete even before the celebrated schism of 1896 between Naumann and the older social-conservative faction of Adolf Stoecker. Thereafter, theological disputes and the lure of *Weltpolitik* effectively diverted attention from any activist social agenda.

Despite references to a host of primary sources, both published and unpublished, Kouri's book is primarily an exercise in synthesis, designed to offer an orientation to the vast secondary literature. Under the circumstances it is probably unreasonable to expect coverage of all relevant studies. Still, although Kouri proves astonishingly thorough in evaluating Anglo-German scholarship, he appears unfamiliar with many significant French and American contributions, among them the work of Ernst Helmreich, Fritz Ringer, Walter Struve, and Rita Thalmann, to cite the most obvious examples. Some readers are likely to find Kouri's emphasis on the feudalized character of the German bourgeoisie more fashionable than convincing. And, although it would not be hard to make a case for his nearly exclusive focus on Prussia, this inevitably causes him to ignore or downplay developments elsewhere, especially in southern Germany, which might mod-

ify his thesis at some points. On a more mundane level, Kouri's discussion anticipates and repeats itself with a frequency irritating in so admirably compact a study.

None of this, to be sure, detracts seriously from the book's basic value. Kouri has provided a perceptive summary of the state of the argument and an obvious starting point for future research. Given the book's rather daunting price tag, one only hopes it receives the use it deserves.

DAVID J. DIEPHOUSE
Calvin College

HORST GRÜNDER. *Christliche Mission und deutscher Imperialismus: Eine politische Geschichte ihrer Beziehungen während der deutschen Kolonialzeit (1884–1914) unter besonderer Berücksichtigung Afrikas und Chinas.* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1982. Pp. 444.

Recently, a number of first-rate studies of the relationship between the Christian missionary enterprise and Germany's overseas endeavors have appeared, for example, Klaus J. Bade's *Friedrich Fabri und der Imperialismus in der Bismarckzeit* (1975) and Lothar Engel's *Kolonialismus und Nationalismus im deutschen Protestantismus in Namibia 1907 bis 1945*. Horst Gründer's Münster University *Habilitationsschrift* is also a significant and welcome addition to the literature. Working from the standpoint of political history rather than institutional, ecclesiastical, or theological development, Gründer examines the close ties existing between the Protestant and Catholic missionary endeavors and the German colonial authorities in Africa and China between the 1880s and 1914. He gives equal attention to the activities of missionary agencies and state organs in the metropole and the colonial periphery and shows convincingly that following the establishment of the overseas empire the mission societies were quickly transformed into national or "colonial" missions.

The author correctly acknowledges that religion has an independent dimension that allows it a measure of autonomy, but at the same time the proclamation of the Christian faith is shaped by the political and social situation in which it operates. He shows that missions followed in the wake of imperialist expansion, depended on the authorities for protecting property and eliminating opposition, and welcomed this assistance in expanding their scope of operations. German missionaries played a key role in the implementation of colonial rule by their creation of economic enterprises, involvement in judicial proceedings, mediation of differences between officials and indigenous peoples, sponsorship of Western-style education that instilled respect for authority, promotion of Christianity as a civilizing

force, and ceaseless propagandizing at home through missionary conferences and publications in behalf of overseas involvement. Thus, missions were not merely the beneficiary of colonial expansion but actually an integral part of the process.

The missionaries who helped secure colonial rule by providing technical and social infrastructures and preaching a "paternalistic developmental dictatorship" unwittingly set up the preconditions for modern nationalism in the areas where they worked. They contributed to the decline of the authoritarian tutelage of the traditional social order, the emergence of hitherto underprivileged or unrecognized groups, and the creation of an educated elite committed to individualism and personal rights. The missionaries thereby advanced the process of modernization. With respect to the humanization of imperialism, they succeeded in ameliorating some of the worst evils by making common cause with the colonial regime and public opinion at home against rapacious settlers and planters, but their protests amounted to little, because these were directed at individual abuses rather than the system itself. Yet their caritative measures did help local people survive wars, famines, and epidemics and enabled the social and cultural reintegration that lies at the root of modern nationalism.

Gründer has left few stones unturned in exploiting the prodigious body of missionary archival material and published works, and his sense of balance is commendable. He is critical of both facile Marxist economic interpretations of imperialism and contemporary nostalgic, utopian views of life in traditional societies. Although I think his evaluation of the humanitarian activities of missionaries may be overly harsh, he has produced a work that no student of imperialism should ignore.

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ROGER CHICKERING. *We Men Who Feel Most German: A Cultural Study of the Pan-German League, 1886–1914.* Boston: George Allen and Unwin. 1984. Pp. xiv, 365. \$29.95.

In view of its 365 pages, this should be a substantial book. And, indeed, it is the first general study of its subject in English since Mildred Wertheimer's durable contemporary treatment of 1924. A number of recent works have shed fresh light on the pan-Germans' activities, and Roger Chickering performs a valuable service in bringing these findings together. He adds a more exhaustive analysis of the league's social make-up and ideology, and it is on the central five chapters (chaps. 4–8) dealing with this inner world of pan-German activity that the book's claims to originality will rest.

Here the analysis is generally well done, particularly the treatment of pan-German sociability, which is consistent with other recent discussions of the radical nationalist public and its conception of patriotic virtue. This is preceded by an exegesis of ideological motifs, a survey of the league's support, an appropriately hesitant foray into social psychology, and a geography of pan-German local organization. Chickering finds that pan-German ideology "reflected the social experience of a group of men who performed custodial roles over culture and authority" (p. 133), whose proneness to radical nationalist militancy originated in various kinds of marginality and the psychological strains accompanying their place in society. Pan-German activists came more from high-ranking but less prestigious sectors of public service and the professions. They were also found more frequently in central Germany than elsewhere, in Protestant administrative centers close to strongholds of the SPD.

Most of this carries conviction. If there is a weakness, it is in the relationship between the ideological and the social analysis. Chickering sees this in terms of "homologies" between "ideological vision" and "social roles" (p. 118). But the burden of explaining such a specific ideological formation as Wilhelmine radical nationalism is surely more arduous than this and requires a more dynamic account of ideological conflict. My own view is that the impetus came from a populist critique of ossified notable politics, and, although Chickering talks of "pressures and strains within the circles of notables" (p. 114), he ignores this more specific ideological contradiction. As a consequence, he is better at exploring the culture of ethnic solidarity than the dynamics of right-wing political mobilization.

The limitations of Chickering's "cultural" approach to pan-German ideology increase the burden on the narrative account of the league's development, and here the chapters are disappointingly routine. They add surprisingly little to what we know about the league's foundation or its role in the right-wing regroupment after 1911-12 and fail to illuminate more obscure episodes, like the nascent radical nationalism of the 1880s, the abortive "National Party" of 1892-93, or the plans for a coordination of patriotic organizations in 1908 (which goes unmentioned). This is partly because some key sources, like the voluminous correspondence of Ernst Hasse and Adolf Lehr and the papers of Theodor Reismann-Grone, have not been exploited. But there is little attempt to place individual pan-Germans politically or even to define the changing political configuration of the pan-German leadership between the 1890s and the ascendancy of Heinrich Class after 1908. Certain salient individuals (like Reismann-Grone or Georg von Stössel) are hardly mentioned except in passing.

Much of the league's developmental history is subtly homogenized, with quotations and references often being drawn indiscriminately from a twenty-year period. This obscures internal differences and their severity, and, although the critical departure of Class's address to the 1903 congress is noted, neither the ructions arising from the Boer agitation nor the disappointments of the 1903 elections are properly related. The discussion of the pan-Germans' emergent "national opposition" is curiously disjointed, with only a page on the Daily Telegraph Affair (p. 222 n.) buried in a downbeat treatment of the rift with the government and removed from the Navy League crisis (pp. 253-62), where it belongs. In consequence, the pivotal importance of events in 1908 is lost. The league's party connections also deserved more extensive treatment. In one conception, radical nationalism was the ideological stalking-horse for a party political coalescence of the right, and the 1903 and 1907 elections acquire a particular interest in this regard. Indeed, before 1908 pan-Germans contributed less by their independent organizational presence than by their agitation of a broader radical nationalist milieu, which coexisted ambiguously with the party political arena. More concrete political analysis was needed to illuminate this question and might have replaced some of the more labored cultural discussion (like the exposition of pan-German imagery or the otiose description of the weather at pan-German outings).

In the end, then, this is a welcome contribution to Wilhelmine political history, but one that, rather surprisingly, does not exhaust the potential of its subject.

GEOFF ELEY
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BERND SÖSEMANN, editor. *Theodor Wolff: Tagebücher, 1914-1919; Der Erste Weltkrieg und die Entstehung der Weimarer Republik in Tagebüchern, Leitartikeln und Briefen des Chefredakteurs am "Berliner Tageblatt" und Mitbegründers der "Deutschen Demokratischen Partei."* In two volumes. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, numbers 54/I, 54/II.) Boppard a/ R.: Harald Boldt. 1984. Pp. xiv, 572; vi, 573-1097.

Theodor Wolff was a man of many talents—essayist, short story writer, novelist, playwright, political activist—who in 1918 became one of the founders of the German Democratic party. But his chief claim to fame and influence was his editorship of the *Berliner Tageblatt*, a position he held from 1906 to 1933, when the paper was first "coordinated" and then killed by the Nazis.

It had been an extraordinary newspaper. "What this paper twice-daily transmits in the souls of its

readers," wrote a contemporary critic, "seems to me the most typical and pure expression of what to some degree can be called the dominant attitudes of our time. [Certainly,] the average foreigner believes that things in Germany are the way the *Berliner Tageblatt* describes them. It is the only paper he knows" (vol. 1, pp. ix-x).

The paper's editorship brought with it a similarly special position in German life. Wolff knew the power elite of Wilhelmine Germany on both a professional and a personal basis. "At night, dinner at Count Hatzfeldt. Among the guests: his charming daughter-in-law, the daughter of Ambassador Tschirschky (nothing of the stiff and boring father about her!), Prince Münster-Derneburg, Georg Wedel, Mumm von Schwarzenstein, . . . a few aristocrats, an old general, Professor Hoetzsch of the *Kreuz-Zeitung*" (vol. 1, p. 255).

This necessarily abbreviated guest list is taken from Wolff's wartime diary. It was found by his son in his Paris apartment in 1970 and has been edited in exemplary fashion by Bernd Sösemann, along with a selection of Wolff's letters and editorials. Such diaries, textually unchanged as this one is, can in the light of hindsight be unkind to their authors. Wolff's is not. The overwhelming impression it conveys is that of Wolff's great good sense. Even in the war's early days, he kept his judgment and sense of balance. If he supported the war, he did so with remarkable restraint. An early editorial sets the tone. "Above all," begins an editorial on July 27, 1914, which closely mirrors an entry in his diary the previous night, "it is necessary to persuade the boys and men who for the last two nights displayed their enthusiasm with such sound effects in Berlin's streets to go and get some sleep. Mixing foreign policy with street noise is a poor idea" (vol. 2, p. 754).

Yet he went along with government policy in the beginning, largely because of his fear of Russia, "the one moral justification for the war," as he put it. But his reservations were always apparent, and he consistently opposed the annexationists, favored peace talks, and, despite the early successes of unrestricted submarine war, remained deeply skeptical about its wisdom.

These were attitudes he maintained despite all pressures, and he knew why. As he recalled in his diary when a government spokesman tried to persuade him to support the high command's position on the submarine war in the summer of 1916, "For twelve hours, I allowed Jagow [in 1914] to talk me into a policy which went against my grain and to defend the ultimatum to Serbia—for only twelve hours, but it has oppressed me forever since" (vol. 1, p. 376).

If anyone then had a right to oppose Versailles, Wolff did—and attack it he would. "What we know so far about the treaty draft is enough to form a

judgment," he wrote in his editorial of May 8, 1918. "It is a document of force, of a policy of subjugation, worlds removed from any League of Nations ideas. . . . The entire treaty is said to consist of ten thousand words. If it remains like anything it is today, one can only say one word: *No!*" (vol. 2, p. 843).

The sad part was that his quiet, rational kind of patriotism had exposed him to bitter attacks in the nationalist press and to the frequent censorship of individual articles as well as an occasional ban of the paper itself. But at least it was not an isolated battle. The diary shows that a number of his friends saw things equally clearly. There was Rathenau, who before 1914 was out called the war "a madness," and Pourtales, who noted how differently things might have turned out if at least the Austrians had had the wit to act quickly and "as anticipated moved on Belgrade the first day." There was Bülow, who as early as Rathenau called the day the war broke out "*ein Unglückstag*," and Zimmermann, who had no use for the annexationist clamor—"Germany's strength is in the cohesion of its people, we've seen this now"—and Bethmann, who in one of his better moments, early in 1915, said about the previous year's war enthusiasm, "What we saw there was crazy, was *hybris*—as though we had to conquer the whole world, as though only we existed."

That they could not change things, that even the best of them could not resist the war's escalation, was something else. Hence, there are also some less endearing entries, whether of Zimmermann trying to shrug off his Mexican telegram—"too bad" it was intercepted—and Bethmann saying that as far as the projected submarine war was concerned, "I don't understand anything about it. It's purely a technical question." It was an attitude Bethmann would change, of course, and, on balance, many of those quoted emerge as men of integrity and intelligence, fully as aware at the time of the problems and dangers as any later historian would be. If their actions would leave them open to criticism, it was not for their lack of understanding but rather for the narrowness of their choices.

Their world, as more than one among them had sensed, would vanish. There was no saying "No" to Versailles; even the *Berliner Tageblatt's* editorials had their limits. And what 1919 left intact, 1933 would destroy. But it had hardly been the worst of worlds, and certainly Theodor Wolff, as these two volumes show, had been one of its finest spokesmen. This is a diary well worth having.

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STEWART A. STEHLIN. *Weimar and the Vatican, 1919–1933: German-Vatican Diplomatic Relations in the*

Interwar Years. Princeton: Princeton University Press. 1983. Pp. xvi, 490. \$50.00.

After the upheaval of the First World War, the Catholic church was anxious to assure its legal position in the insecurely established republics of Central and Eastern Europe. Rome's overtures were particularly welcomed in Germany, where it was hoped that the Vatican could be useful in overcoming the consequences of defeat. In this fascinating account, Stewart A. Stehlin describes the close relationship between the Vatican and the Weimar Republic. Vatican archives are not open for these years, but he has amply compensated for this by exhaustive use of public and diocesan archives in Germany and in six other countries. The result is a detailed record that will interest not only historians of Germany and the church but also everyone concerned with the diplomacy of the interwar period.

A vital concern of both the Vatican and the German government was the adjustment of diocesan boundaries to conform to Germany's postwar frontiers. German policy was to facilitate eventual restoration of territory by preventing the absorption of German-speaking congregations into existing French, Belgian, Polish, and Lithuanian dioceses. The Vatican proved remarkably accommodating on this question. Later, in the 1923 Ruhr crisis, Germany asked for and received Vatican sympathy and, thus, earned international respectability at the expense of the French invaders, causing the leaders of the Third Republic to regret their country's lack of representation in Rome.

In return for playing honest broker in Germany's rehabilitation, the papacy hoped to secure official legal status in the republic for the church, which had not been possible before the war, when most German states had ties to the evangelical churches. Negotiations for such legal guarantees resulted in the signing of concordats with Bavaria (1924), Prussia (1929), Baden (1932), and the Reich (1933). The state treaties, except for the Bavarian, were less than satisfactory to the Vatican because they did not include assurances for the maintenance of Catholic public schools. All attempts to draft a national treaty before 1933 foundered on this issue. Stehlin makes this clear, but he fails to provide an adequate explanation of the existing church-state relations in the republic. Although information the reader needs is scattered throughout the book, it could have been usefully summarized in an early chapter.

Educational concessions withheld from the church by the republic were granted almost casually by the National Socialist government in 1933, doubtless because there was no serious intention of honoring them. Stehlin rightly chooses to place the 1933 treaty in the context of fifteen years of concordat negotiations in Germany and other countries,

but readers may be disappointed by the scant sixteen pages allotted to it. The literature on this controversial subject is extensive. But most of it is in German, and a detailed account in English would have been a service to those who do not read German. Stehlin minimizes the issue by saying that the Nazi regime "was soon to disregard the concordat, treating it as a mere scrap of paper" (p. 452), but in fact the treaty was declared by German courts in 1957 to be law, and it has determined educational policies in the Federal Republic. It is possible that, with the changed demography and climate of opinion in postwar Germany, similar concessions could have been won from the Bundestag, but this is by no means certain. The *Reichskonkordat* was, indeed, the contractual agreement that the church had been working toward since 1918. Consistency on the part of the German government is also evident. Hitler, like Social Democratic and Catholic Centrist chancellors before him, used the relationship with the Vatican to obtain "recognition for the regime by the greatest moral force in Europe" (p. 447).

ELLEN L. EVANS

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HORST DICKEL. *Die deutsche Aussenpolitik und die irische Frage von 1932 bis 1944*. (Frankfurter Historische Abhandlungen, number 26.) Wiesbaden: Franz Steiner. 1983. Pp. x, 254. DM 68.

This book advances the idea that, by examining the relatively minor question of German policy toward Ireland in the years of the Third Reich, it is possible to gain a better understanding of both the goals and the process of foreign policy decision making under Hitler. Although Horst Dickel has studiously covered a great deal of material and used extensive files, especially those of the German consulate in Dublin, the promises of the book remain unfulfilled.

Instead, this is essentially a rather pedestrian study of Irish developments following the inauguration of Eamon de Valera in February 1932 and the attendant alterations in German-Irish relations. In fact, as Dickel shows clearly, relations did not change very much. The German foreign office was primarily interested in securing an understanding with Great Britain and therefore opposed Irish suggestions for closer cooperation out of a fear that such moves might jeopardize the friendship of London. Following interpretations from a number of recent scholars, Dickel stresses that even Hitler did not introduce a call for a more dynamic approach. On all levels of German policy making there was general agreement: Ireland could be useful, particularly as a source of embarrassment for Britain and a justification for German activities in Czechoslovakia. For how could Britain oppose Berlin's hard line

toward persecution of Germans by Prague when London was unwilling to grant full independence to Ireland? But such considerations were merely tactical. There was never much of a place in Germany's strategy—either political or military—for Ireland.

Despite a recent flurry of books about the IRA and its connections to Germany and about the presence of German spies in Ireland, Dickel generally plays down the importance of these activities. He finds that no serious attempt was made either by the official German government or by Nazi party groups (including the SS) to use the IRA for terrorist activities or to bring down de Valera's government in favor of a more pro-German administration. Throughout the 1930s German policy stressed the maintainance of proper, but not close, relations with the Irish Republic.

This becomes clear when Dickel turns to the various problems created by the outbreak of war and Ireland's declaration of neutrality. He confirms the inability of the German navy to see any advantage in closer ties with Ireland and shows that, militarily, Ireland's neutrality was the best the Germans could hope for. Attempts to use Irish bays or coves as shelters for U-boats and raiders would only force Britain to occupy the whole island, instead of just the North. And the German navy could provide no military help to Ireland in the eventuality of such a British attack.

The most interesting discussion in this section of the book concerns the role of the United States. Dickel argues forcefully that the major factor dominating German (as well as British) relations with Ireland was apprehension concerning the United States. Berlin hoped that continued controversy between de Valera and the British government would guarantee America's neutrality. The Irish-Americans, they argued, would never enter a war against Germany on the side of a Britain that continued to refuse the unification and full independence of Ireland. Thus, German diplomats (and soldiers) urged strict respect for Irish neutrality, confident that Britain would sooner or later be forced to take actions that would violate Irish neutrality and thus keep the United States out of the war.

Nevertheless, a number of independent agencies in Berlin (including the *Abwehr*) pursued an independent policy, particularly after the fall of France. These groups sent in spies, maintained contact with IRA terrorists, and sought renewed contact with Irish politicians. Dickel shows that German diplomats opposed such activities and eventually prevailed. These conclusions, however, are not surprising. The reality of the wartime situation prevented Ireland from being of any use at all to Germany, and this was fully confirmed following Pearl Harbor. Moreover, Germany's impotence to help Ire-

land was also demonstrated, particularly after the military defeats of 1942 and with the subsequent growing pressure on Dublin to join in the war.

Although Dickel does not make it a specific theme of his study, the heart of the Irish Question was unification. De Valera was too much of a politician to believe that unification could come in the face of British opposition; thus, successful links between Dublin and Berlin could be created only after unification, not before. The Second World War, therefore, gave very little opportunity for either Germany or Ireland to draw closer together. Tragically, despite de Valera's reasonable refusal to depart from his moderate line and despite Berlin's clear lack of interest in expanding the war to include Ireland, 1945 brought no closer the solution to the Irish Question.

Although this book requires no great adjustment in existing interpretations of either German policy or the Irish Question, it is a well-researched retelling of the story. Unfortunately, at times the complexities almost overwhelm the narrative, and Dickel's insistence on breaking each chapter into tidy subdivisions (the roll of the IRA, German public opinion, Irish appeasement, and so on) does not make for smooth reading. Moreover, the text is flawed by a number of careless misprints. Frequently, his English quotations lack proper punctuation; the result is awkward, sometimes incomprehensible, sentences. And the proofreading was less than perfect. Thus, Father Coughlin (without a Christian name) turns up properly in a footnote but becomes McCoughlin in the text on the same page; the error is then repeated in the index.

JOHN L. HEINEMAN
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THOMAS CHILDERS. *The Nazi Voter: The Social Foundations of Fascism in Germany, 1919-1933*. Chapel Hill: University of North Carolina Press. 1983. Pp. 367. \$34.00.

Thomas Childers's *The Nazi Voter*, like several other recent contributions to the literature on Weimar electoral trends, questions the traditional interpretation that Nazism was essentially a lower-middle-class movement. The author sets out to examine the Nazi constituency to discover "how it was formed, from which social group, under what conditions, and with what promises" (p. 14). Childers finds that the electoral support for the Nazi party (NSDAP) came from a much more diverse social milieu than once believed and that it changed significantly with shifting economic and political conditions.

To determine the social origins of Nazi voters, Childers employs multivariate regression analysis,

using as his socioeconomic data base the 1925 census figures for almost five hundred towns and rural counties throughout Germany. These data are then applied to all Reichstag elections between 1924 and 1932 to obtain coefficients relating Nazi votes to five major social and cultural categories: the old middle class, the *Rentnermittelstand*, the new middle class, the working class, and religious groups. This technique allows Childers to demonstrate both the realignments of electoral patterns and the disintegration of traditional bourgeois electoral loyalties that followed the inflation and stabilization crises of 1923–24. "Without the destabilization of traditional voting allegiances within the middle-class electorates," notes the author, "the spectacular rise of the National Socialist fortunes after 1928 is hardly conceivable" (p. 127).

Childers supplements his quantification methods with traditional literary and archival sources to describe the appeals of all major Weimar parties to the various segments of society that were experiencing a variety of socioeconomic maladies. These traditional sources include major political pamphlet collections in several German archives and, for the Nazi party in particular, the *Völkischer Beobachter*, the Abel collection, and the NSDAP Hauptarchiv (primarily for Josef Goebbels's directives in 1932). For basic information about various German social groups and their economic difficulties during the Weimar Republic, the author leans heavily on standard secondary literature.

In the end, Childers finds that the *völkisch* voters of 1924 were malcontents from "all walks of life." Not until 1930 did the Nazi party penetrate significantly all major sections of the middle-class electorate and become "the long sought-after party of middle-class integration" (p. 178). The nucleus of support for the Nazis between 1924 and 1932 came from the old middle class. Artisans, farmers, and small shopkeepers, who most feared the social and economic consequences of modern industrial society, were attracted by the NSDAP's antimodernism. Hitler's party also gathered protest votes from pensioners, widows, civil servants, and, to a lesser degree, white-collar employees who reacted to specific economic crises. Like Richard F. Hamilton, Childers notes the strong support for the Nazi party in the early 1930s from established social circles, affluent urban districts, and older voters. Interestingly, however, the Nazi/white-collar coefficients were always much weaker than those for other segments of the middle class. *The Nazi Voter* also confirms what Werner Stephan noted fifty years ago: that industrial workers and Catholics were least likely to support Hitler's movement at the polls.

Although much of Childers's research is thorough, there are minor weaknesses. Childers did not consult the National Archives microfilm T-580 col-

lection (or the German originals), which contains important Nazi propaganda material. This material includes the correspondence of Heinrich Himmler, the coordinator of the party's propaganda efforts during much of the late 1920s. With the exception of one citation from Franz-Josef Heyen's book, the author did not use police reports to illustrate the grass-roots response to Nazi propaganda. Finally, Childers is confused about the Nazi party's "urban plan." Although he notes that in early 1925 "the party adopted what came to be known as 'the urban strategy,' . . . calculated to attract the support of the urban working class" (pp. 119–20), he maintains several pages later that an urban plan advocated by Goebbels "was not implemented in 1926" (p. 122). He cites Dietrich Orlow to support this latter statement when, in fact, Orlow views Goebbels's appointment as Gau leader of Berlin in late 1926 as symbolic of the party's support of the urban plan. There are also several printing errors. For example, French troops marched into the Ruhr in 1923, not 1924 (p. 50).

Nevertheless, this book is an innovative contribution to our knowledge of Weimar voting patterns. It restates with greater clarity much of what we know and puts to rest the argument that an undifferentiated middle class, driven by antimodernism, voted for Hitler.

JOHN PETER HORST GRILL
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SARAH GORDON. *Hitler, Germans, and the "Jewish Question."* Princeton: Princeton University Press. 1984. Pp. xiv, 412. Cloth \$40.00, paper \$14.50.

Sarah Gordon's *Hitler, Germans, and the "Jewish Question"* deals with the background of the Nazi persecution and murder of European Jews as well as German reactions to the persecutions and murders. In doing so, it asks us to reexamine basic assumptions that have shaped our understandings of the relationship between Germans and Jews, especially during the Hitler era. These include assumptions about who supported Hitler and why, about the role of anti-Semitism in the rise of the Nazi party, about Hitler's role in the process that led to mass murder, and about German reactions to those policies.

Gordon finds anti-Semitism in nineteenth-century Germany, for example, to be no more, and possibly less, virulent than elsewhere in Europe. Her introductory chapter on the remarkably successful assimilation of Jews into the society of the Second Reich notes, nonetheless, that by the beginning of this century Jews were increasingly concentrated in visible middle-class endeavors such as banking and retail and wholesale trade; in professions such as

medicine, law, journalism, and teaching; and in the theater, the arts, and film. Those in the arts and journalism, she suggests, were often likely to be critical of traditional values and thus easily labeled "un-German." Likewise, Gordon notes, by 1929 Jews in Berlin were estimated to have per capita incomes twice those of non-Jewish Berliners.

As for the role of anti-Semitism in bringing Hitler to power, Gordon believes it to have been minimal. Those who voted for Hitler were attracted to him, she argues, less because of his anti-Semitism than because of his other positions, particularly his claim after 1930 to be the last bulwark against communism. Nor does Gordon accept the traditional view that the first Nazi voters came primarily from the lower middle classes. As evidence she cites the findings of Richard F. Hamilton, which suggest that Nazi support, although it came from across the social spectrum, came particularly from the upper and upper-middle classes.

How does Gordon account for the horrors visited by the Nazis on the Jews? She begins her answer with a lengthy chapter on Hitler's ethnic theory. Here she examines the world view that saw all of history as the product of unequal races struggling for living space and world domination. Inevitably, in Hitler's perception, this process had to lead to a struggle between superior Aryans and inferior Jews. The necessary result was the Nazi's Final Solution to the Jewish question.

Gordon sees Hitler's role in defining this world view and in making the decisions that led to the Final Solution as decisive at every stage—an interpretation that places her on the intentionalist side of the gradually waning intentionalist-functionalist controversy first defined by Tim Mason. Gordon believes that it was in a 1936 conversation with Hermann Rauschning that Hitler let slip for the first time his intention to kill the Jews.

The truly original part of the book concerns the pockets of opposition to the persecution of Jews that Gordon finds in the military, the bureaucracy, the press, and even the Nazi party itself. Basing this part of her study, which she refers to as exploratory rather than exhaustive, on Gestapo files, she suggests that among the lower levels of German society there may have been more resistance to the persecutions than we have been led to expect (or at least a lack of enthusiasm for them). Hence, the importance of Hitler and the rabid anti-Semites in keeping the Final Solution on track.

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REGINA BRUSS. *Die Bremer Juden unter dem Nationalsozialismus*. (Veröffentlichungen aus dem Staatsarchiv

der Freien Hansestadt Bremen, number 49.) Bremen: The Archive. 1983. Pp. 341.

The publication of general accounts of Jewish policy in the Third Reich by Uwe Dietrich Adam and Helmut Genschel has been accompanied by a proliferation of regional studies of the German Jews between 1933 and 1945. A new and distinguished contribution to this list is *Die Bremer Juden unter dem Nationalsozialismus* by Regina Bruss. The massive documentation of the book is based on the astonishingly rich records of the Bremen senate and chamber of commerce, which survived the war.

The author intends to encompass the total experience of the Bremen Jews. She presents an excellent account of the exclusion of the Jews from the civil service, the professions, and economic life. In this analysis the quantitative data are enlivened by a wealth of evidence on the experiences of individual businessmen and professionals. She describes in rich detail the economic, social, and psychological effects of persecution on the Jews. A lengthy and precise account of *Kristallnacht* in Bremen shows that the scale of violence to human life and property went far beyond the burning of the synagogue and discloses how the SA troops were able to terrorize the Jewish population in so large a city in so short a time.

This study of Bremen substantiates the view that Jewish policy in the Third Reich was not implemented uniformly and that the pace of discriminatory measures and the intensity of violence depended on local conditions. In discussing the exclusion of the Jews from the legal profession, Bruss identifies the beginning of a Bremen tendency to take hasty and independent initiatives without waiting for the Reich government to issue regulations. As early as May and June 1933, the commissioner of justice of the Bremen senate introduced measures to prohibit Jews from practicing law, before any ordinance from Berlin authorized the state governments to take such steps. The Reich minister of justice soon recognized that the execution of the law concerning admission to the legal profession had been pushed too far and attempted to halt measures against Jewish lawyers in Bremen and elsewhere.

The Bremen senate moved to boycott Jewish businesses, furthermore, long before the Reich government introduced measures to Aryanize the economy. In March 1933 the senate adopted a resolution ordering the departments of the city's administration to make no further purchases from Jewish firms. Unauthorized local actions in Bremen annoyed the minister of economics on many occasions. He criticized the police for not protecting Jewish stores from a boycott during the Christmas season of 1934. The mayor was rebuked for excluding Jewish merchants who came from Poland from participation in the trade fair of 1935.

The author's explanation of the initiatives that the senate took to hasten the exclusion of the Jews is not as probing as one would expect. She mentions too briefly the influence of local pressure groups composed of small retailers and tradesmen, whose aim was to eliminate Jewish competitors. She suggests that the prudential anti-Semitic measures of the Reich ministers presupposed conditions very different from the aroused state of public opinion in Bremen produced by the party's press. Citing the monthly reports on public opinion made by the party's district propaganda office, she notes public demands for the exclusion of Jews from the municipal swimming pools and for the separation of German and Jewish pupils in school. She does not explain how these reports measured popular sentiment and which groups were making the demands. A comparison between Bremen and other cities, where the autonomy and discretion enjoyed by the local government and party leadership led to a milder treatment of the Jews before November 1938, could provide some answers to the question of why Bremen set a swift pace in the execution of the Jewish policy.

MARJORIE LAMBERTI
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PETER R. BLACK. *Ernst Kaltenbrunner: Ideological Soldier of the Third Reich*. Princeton: Princeton University Press. 1984. Pp. xiv, 348. \$32.50.

Of the twenty-two Nazis tried for major war crimes at Nuremberg, Ernst Kaltenbrunner, leader of the Austrian SS after 1937 and head of the *Reichssicherheitshauptamt* (RSHA) from 1943 to 1945, has been one of the few to escape biographical scrutiny. This gap has now been filled by Peter R. Black, a historian with the Office of Special Investigations in the Department of Justice.

Ernst Kaltenbrunner is an almost purely political biography; Kaltenbrunner's rather ordinary ancestry and childhood is covered in only seven pages. Somewhat more space is devoted to his chemical and legal studies at the University of Graz, where his membership in the nationalistic and anti-Semitic *Burschenschaft* "Armenia" had an important ideological influence on his later career as a Nazi.

After three boring years as a lawyer, Kaltenbrunner joined the Austrian Heimwehr in the summer of 1929. Its ambiguous attitude toward an Anschluss and its lack of a clear-cut *völkisch* ideology caused him to join the Austrian Nazi party in 1930 and the SS in the following year. Heinrich Himmler elevated him to head of the Austrian SS in 1937 because of his skill in personal politics, his political contacts in both Germany and Austria, and his expertise in intelligence operations. These same qualifications

helped lead to Kaltenbrunner's appointment as head of the RSHA six years later.

In this latter capacity Kaltenbrunner was occasionally involved in the treatment of foreign labor from Eastern Europe, the persecution of churches, and the deportation of Jews to concentration camps. He directed the commission that investigated the July 20 attempt on Hitler's life. He also managed to establish an "alliance" with Martin Bormann and was thus one of the five most powerful men in Germany during the last six months of the Third Reich. His loyalty to Hitler and to National Socialism remained unswerving until his death by hanging in October 1946.

Black's biography is by no means revisionist; he sees Kaltenbrunner's conviction at Nuremberg to be fully justified. Nevertheless, he denies that the Austrian can be easily categorized as a "monster-criminal-psychopath" (p. 277) and points out Kaltenbrunner's efforts to intercede on behalf of ideological opponents of the Nazi regime—as long as they were not too dangerous and were from his native Upper Austria.

The author's narrowly political and ideological approach to his subject results in our learning next to nothing about Kaltenbrunner's personal life and its possible impact on his political career. Likewise, Black passes over numerous opportunities to compare Kaltenbrunner's childhood and education with those of other Nazi leaders.

Even so, this is an objective and scholarly study in which Black is careful to provide adequate background information on the politics of the Austrian and German Nazi parties. His research is nearly exhaustive and includes interviews with some of Kaltenbrunner's close relatives and many previously overlooked documents, such as a "memoir" written by the Nazi leader just before his death. Scholarly aids include frequently annotated footnotes, a twenty-page bibliography, a twenty-six-page index, and eight pages of interesting photographs.

BRUCE F. PAULEY
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CHRISTA KAMENETSKY. *Children's Literature in Hitler's Germany: The Cultural Policy of National Socialism*. Athens: Ohio University Press. 1984. Pp. xv, 359. \$32.95.

To educate the youth of a nation to become future leaders of a Thousand Year Reich was an assignment of great importance. In a significant study based on years of archival research and embracing a wealth of primary sources, Christa Kamenetsky demonstrates exactly how the Nazis indoctrinated "the young nation." She commences with an analysis of Nazi literary theory and cultural policy and shows

how folktales, Norse mythology, Germanic epics, the classics, and even picture books were married to Nazi ideology. The author next focuses on the uses and adaptations of children's literature in official primers, textbooks, puppet shows and plays, and, most significantly, Hitler Youth ritual. A discussion of the struggle for power between Joseph Goebbels and Alfred Rosenberg for control of the youth cultural realm, as well as a discussion of censorship policies and curricular and library reform, concludes the book. Copious illustrations from several youth genres lend depth and understanding to the work. Footnotes at chapter endings ease the labor for careful readers. Attention to technical detail by the Ohio University Press results in a beautifully produced book, a richly rewarding and informative historical tapestry.

According to the author, ideology and control were the two wings of the Nazi coordination of youth and the state, and children's literature was to function as the beacon of inspiration to the Volk community and the nation. In Kamenetsky's words, "outside of this goal, neither children nor books were thought to have a purpose and justification for existence" (p. 311). Consideration of the fulfillment of the individual child was thought to be a liberal perversion. A philosophy of heroic death became an end in itself.

Hans Friedrich Blunk, president of the Reich Literature Chamber, saw his role as that of a cultural gardener who "pulled out the weeds from a healthy bed of flowers." Reich Education Minister Bernhard Rust, armed with the theories of Ernst Krieck and Severin Rüttgers and prodded by Rosenberg, instituted a rigid unified curriculum throughout the network of German schools. All published works extolled the virtues of the Nazi world view—heroism, readiness for self-sacrifice, courage, a sense of duty, honor, and loyalty. Love for the German Volk and nation and its God-sent Führer—but contempt for "Jewish subhumanity"—was a moral imperative. Boys were to be future soldiers; girls, mothers and facilitators. Blood and soil, the eternal German landscape, and an ennobled peasantry became symbols of a quasi-religious mysticism. At every juncture, nature was a grand teacher. The past and present were one, and mythology joined history as preceptor of the heroic life. In such a world, Thor became a pioneer god for settlers in the East, and Snow White's reawakening took on world historic importance.

Kamenetsky's work is not without its faults. There are serious bibliographic omissions in the chapters dealing with the politics of literary control. Organizational weaknesses are revealed at several points, and stylistic infelicities detract from the book. Nevertheless, *Children's Literature in Hitler's Germany* is

highly recommended for the insight it gives into a neglected area of youth culture in the Third Reich.

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SAUL S. FRIEDMAN. *The Oberammergau Passion Play: A Lance against Civilization*. Foreword by EMIL FACKENHEIM. Carbondale: Southern Illinois University Press. 1984. Pp. xxvii, 270.

To write in the post-Holocaust era a "detached" book on the topic of German anti-Semitism is an impossible task that should actually be suspect. This study of the Oberammergau Passion play is not detached. Saul S. Friedman has written a persuasive and very readable book, based on numerous personal interviews of actors and directors of the play, of Christian theologians in different countries, and of representatives of major American Jewish organizations, as well as on other pertinent sources (though the author does omit some major background studies). Despite, or perhaps because of, his commitment, it is one of the best books of its kind on this limited, but still important, contemporary topic.

In the words of the Jewish historian Howard Sachar, this Passion play, "legitimized by its putative religiosity . . . has remained one of the few 'respectable' anachronisms of folkloristic anti-Semitism in the Western world." The play was first staged in 1634, during the Thirty Years' War, when the plague swept over the village, claiming many victims. The survivors, out of gratitude, vowed then to enact a Passion play every ten years. While the inhabitants of Oberammergau have been praised for their piety and fidelity in carrying out their vow, the play has been criticized by Jews as well as by Christians. Though revised periodically, it still contains, in the judgment of its critics, serious theological and historical flaws and an unquestionably anti-Semitic thrust.

In the post-Holocaust era the villagers of Oberammergau have denied such charges and have even claimed that their Passion play was a "hymn of reconciliation"; this has been sharply disputed by Jews and prominent Gentiles, including noted American scholars. Hitler himself appeared in Oberammergau on August 13, 1934, and eight years later he praised the play thus: "Never has the menace of Jewry been so convincingly portrayed as in this presentation." Nevertheless, during the American occupation the Oberammergau Passion play reemerged. John McCloy has reported his and Konrad Adenauer's attendance of the play in 1950 and the remark by the latter, apparently made uncomfortable because of its virulent anti-Semitism: "Das war zuviel" (p. 145).

The author zeroes in on the stubborn provincialism, insensitivity, and plain anti-Semitism of the villagers of Oberammergau who resisted all entreaties and admonishments of the postwar German authorities and the Roman Catholic hierarchy and ignored the resolutions of Vatican II on deicide. Since the changes made in the play after the war have been few and insignificant, the Passion play, in the words of the author, is, as Nietzsche said of Wagner's *Meistersinger*, a "lance against civilization."

Considering it most unlikely that the villagers will radically change the present version of the Passion play, Friedman makes a number of other suggestions, many of which have been made before: Eliminate references to the Jews as a race; place Jesus clearly within a Jewish religious and historical milieu, making clear that he was a Jew; delete gratuitous insults against Jews; play down the greedy aspects of Judas's nature; emphasize continuity of the Old and New Covenants by adhering more closely to Luke; delete the "insidious blood curse" of Matthew and at least half of the screams for Jesus' death; include Christ's lines of forgiveness offered from the cross (lines that have "notably been absent" from the traditional Oberammergau play) (pp. 185–86). In conclusion, the author regrets that half a million tourists in the summer of 1984 will "suffer more unhistorical kitsch" (p. 194).

ALFRED D. LOW
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MARTIN MÖLLER. *Evangelische Kirche und Sozialdemokratische Partei in den Jahren 1945–1950: Grundlagen der Verständigung und Beginn des Dialoges*. (Göttinger Theologische Arbeiten, number 29.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 265. DM 50.

This is an important work that makes a substantive contribution to existing literature on the church and the Social Democratic party in Germany. Originally a dissertation at Marburg, this well-written, cogently argued, and well-researched study is based on extensive archival materials, a broad array of published sources, and many secondary works. The only weakness in Möller's bibliography is his failure to consider numerous (and relevant) English-language studies, for example, W. R. Ward's *Theology, Sociology, and Politics: The German Protestant Social Conscience, 1890–1933* (1979).

The title and subtitle aptly and accurately describe the contents of the study, but the subtitle, *Grundlagen der Verständigung und Beginn des Dialoges*, represents the heart of his analysis. The work is divided into three sections. The first (after a short introduction that discusses methodology and sources) is concerned with the Protestant church in

Germany and its checkered relationship with social democracy. Möller briefly recounts the events of the Weimar and the Nazi eras and then emphasizes the emergence of new attitudes toward socialism in the years immediately following the collapse of the Third Reich. The second section follows the same pattern but from the reverse perspective of the Social Democrats; out of necessity, perhaps, he appends a short discussion on the Bad Godesberger Program of 1959. The last and most crucial phase of his study is on the initiatives and contacts from both sides that led to the beginning of dialogue. I am not absolutely sure, even after a careful reading of the text, why Möller chose to end his study at 1950, unless it was because of the constraints of space—it is fairly long by the standards of most German dissertations—or because he saw the period 1945–50 as a fruitful one in the history of those two institutions. But Bad Godesberg was a crucial moment, and perhaps that would have been a more logical stopping point.

Most significant, this treatise is a history of ideas—a study of important individuals (Martin Niemöller and Kurt Schumacher, among many others) and crucial moments in the history of the two institutions (such as the church conference in Treysa in 1945 or meetings between church leaders and Social Democrats in Detmold in 1947 and Darmstadt in 1950) that led to a growing dialogue, understanding, and acceptance by many on both sides of each other's positions. The author quotes extensively from the sources, but they are well integrated into the text. Evidently, the author's "given" assumptions are that the chief causal factors are the collapse of the Nazi state, the rejection by the Social Democrats of Russian-style authoritarian communism, and the growing acceptance of democratic ideas in Germany. But if anything these assumptions are understated, leading the reader to be a bit quizzical about his underlying interpretations. The conclusion is weak; it would have been better had it tied the various threads of the book together in a more comprehensive manner.

JOHN C. FOUT
Bard College

UDO WENGST. *Staatsaufbau und Regierungspraxis, 1948–1953: Zur Geschichte der Verfassungsorgane der Bundesrepublik Deutschland*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 74.) Düsseldorf: Droste. 1984. Pp. 351. DM 64.

Ever since the early 1970s the process of postwar German democratization, its successes and lost chances, has held a special fascination for the youngest generation of West German historians.

Udo Wengst's book is very much in that tradition. But with the new, though still limited, access to records from the early Federal Republic, his and other recent studies have notably shifted to the question of democratic state building. Wengst's focus is on the practical institution-building process as a combination of the creation of theoretical democratic structures in the constitutional assembly and the actions of the first officeholders of 1949–50. For historians this is an attractive approach, because it stresses the unique contributions of individuals without losing sight of the general institutional framework. In the particular context of post-1945 Germany it also means, however, that the towering figure of the early Federal Republic, Konrad Adenauer, moves into an all-dominating position. Whether this concentration on Adenauer, which has been typical for most general studies on the years after 1948, can ultimately be justified or whether it is simply due to traditional perspective and the extraordinary wealth of sources on Adenauer at the CDU-Archives and the Adenauer House is still uncertain. At present, this mainstream interpretation still awaits a serious revisionist challenge.

Wengst's work in the recently opened records, especially in the archives mentioned above, permits him a number of original and fascinating insights into the establishment of the first Adenauer government beyond the early negotiations in Rhöndorf previously described by Rudolf Morsey. Of particular interest is the extent of continuity Wengst is able to show for the personnel of the new ministries from the old National Socialist ministries, especially the Interior and Economics ministries. In an attempt to rid himself of apparently ineffective bizonal administrators and Social Democrats, Adenauer used a group of officials from the former Nazi Reich interior ministry to advise him on government staffing. These men naturally moved their old associates into leading positions in the new administration with the result that over one-fourth of the department heads in Adenauer's ministries were former Nazi ministry bureaucrats, including, of course, his closest advisor, Hans Globke. In the Bundestag, however, men who had begun their political careers in the Weimar Republic and had remained attached to its democratic ideals predominated.

The first and last parts of the book on the zonal and bizonal institutions of 1945–48 and on the government, parliaments, and court as they functioned after 1949 offer fewer original insights. The former is an intelligent synthesis of the key works on the occupation years, which is notable mainly for its strong emphasis on the growing power and importance of the emerging political parties. In the latter section the author had to rely generally on well-known sources in the Federal Archives and on memoirs, because he was able to gain only very

limited access to the vital chancellery records. Still, Wengst's summary of the celebrated political events, tensions, and personal relationships of this early Adenauer period makes for fascinating reading, and the whole book can be highly recommended as a handy reference work for the entire West German state-building process of 1945–53.

DIETHELM PROWE
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ANTONI WŁADYSŁAW WALCZAK. *Dylematy i obsesje jedności i podziału Rzeszy XIX–XX w.: Polityka ogólnoniemiecka RFN, 1949–1969* [Dilemma and Obsession of Unity and Division of the Reich, Nineteenth and Twentieth Centuries: The All-German Policy of the FRG, 1949–69]. (Studium Niemcoznawcze Instytutu Zachodniego, number 37.) Poznań: Instytut Zachodni. 1982. Pp. 402. 250 Zł.

This book is a comprehensive examination of the successes and failures of German domestic policies between 1949 and 1969 that were oriented toward reunification. Although, as Antoni Władysław Walczak recognizes, German endeavors were not decisive for the solution of the problem, they nevertheless formed a significant part of the overall activities focused on the question of German unity. In fact, Walczak claims, the fiasco of unifying attempts can largely be ascribed to the misguided tactics of West German leaders, especially those of Konrad Adenauer.

Walczak's chain of reasoning starts with an inquiry into conditions that existed in the divided Germany of the nineteenth century. Showing *passim* that the dismemberment of the Third Reich is not a unique phenomenon in German history, he concludes that the main factor contributing to the unification of 1871 was the military force as demonstrated by the *Einigungskriege* against Denmark, Austria, and France. In Walczak's view, the awareness of this historical fact exercised an overwhelming—and detrimental—impact on the behavior of West German leaders during the first twenty postwar years. By promoting virulent anticommunism, aggravating international tensions, questioning the European status quo, and constantly propounding the "German question," they expected to achieve by *tour de force* the unity of all German lands.

But this method, Walczak contends, was totally counterproductive. It helped unite, to be true, the three Western zones and restore a sovereign status to truncated Germany, but at the same time it deepened the chasm between the two Germanies and contributed to the breaking of bonds with the Soviet zone. A "provisional" division of the country was becoming a stable "definitum." More astute German leaders, including Adenauer, were con-

scious of these undesired results, but even they expected that the creation of two German states was only a temporary solution, an *interim Lösung*. They hoped that some day in the future all German territories would be consolidated again, with the Federal Republic acting as the nucleus of the reborn Reich.

Walczak, however, does not entertain much hope for German reunification. He believes that the "doctrine of postponement" that governed Allied decisions regarding the fate of Germany in the last years of World War II has lost its validity. The existence of two sovereign states, both members of the United Nations, proves it quite convincingly. If, however, somebody still thinks in those "anachronistic terms," he must be prepared for dramatic international conflicts that would very likely undermine the existing European stability.

Readers may agree or disagree with Walczak's conclusion and question the line of his reasoning. Their contention will be well supported by the author's general disapprobation of Western policies (is only the West to be blamed for the Cold War?) and rather cavalier treatment of some details, such as the participation of East German delegates in the 1947 Munich conference (pp. 182–87) and the inception of the "air-bridge" to West Berlin in 1948 (p. 148). Probably, these flaws can be ascribed to the need of satisfying the almighty censors, yet they somewhat undermine the objectivity of this otherwise valuable book. The volume is supplemented by a rich bibliography composed mainly of German sources, a short German-language summary, and an extensive index of names.

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ENNO E. KRAEHE. *Metternich's German Policy*. Volume 2, *The Congress of Vienna, 1814–1815*. Princeton: Princeton University Press. 1983. Pp. xi, 443.

In 1963 Enno E. Kraehe published the first volume of *Metternich's German Policy*, which was subtitled *The Contest with Napoleon, 1799–1814*. He promised a second volume to carry the study to 1820, and two decades later we have it. The reality both diminishes and expands the promise: diminishes it, because Kraehe restricts himself to the period of the Congress of Vienna and makes no more promises, and expands it, because he has perfected his method and plumbs his subject so deeply.

His method is straightforward, "old-fashioned," and refreshing: he writes narrative history and "seeks to relate the events of a period, both physical and mental, to each other rather than to norms imported from afar" (p. viii). He narrowed his

chronological frame to 1814–15, because he found that even the best studies of the congress (those of Webster, Griewank, Nicolson, Kissinger) suffer from contemporary preoccupations. He does not mean to replace these studies (his prime concern remains Metternich's German policy), but his method requires him to set the fuller context carefully and so tempts us to claim more for his achievement than he in fact attempted. It is enough to say that, in tracing the complex and confusing process by which Metternich brought the Germanic Confederation into being, Kraehe has given us a wise and vivid description of the congress in operation.

Metternich's goal was the creation of an independent center against the efforts of either France or Russia to establish hegemony. He sought support for his policy from the "conservative powers," those that desired a stable international order, which primarily meant England. He had hoped that a more restrained Napoleonic France might be incorporated into the system, but when he finally (perhaps belatedly) saw this hope was in vain, he accepted the necessity of supporting the Bourbon Restoration. His German policy was the obvious corollary of his European policy: he sought a confederation of German states that would avoid the dangers of a restoration of the old empire, to him a futile exercise in nostalgia, and of a Germany divided into regional leagues that would inevitably lead to Prussian domination of North Germany. To achieve his goal Metternich needed the cooperation of Prussia, the very power he sought to restrain.

Two relationships are at the center of the narrative: that between Metternich and Tsar Alexander of Russia and that between Metternich and Karl von Hardenberg, the Prussian chancellor. Kraehe emphasizes that both of Metternich's protagonists acted from assessments of the needs of their own states—as did Metternich. They differed from him principally in their failure to recognize the needs of other states and their lowered concern for the balance of Europe. That Metternich possessed these qualities explains his success in creating a Germanic Confederation consonant with Austria's needs.

Kraehe concludes that Metternich succeeded in his policy, but Kraehe's Metternich is not the "Coachman of Europe," dominating and directing the other powers. Kraehe delineates the limits of Habsburg power and the liabilities of Austria's position; he notes the flaws in Metternich's vision, the missteps in his policy, and the domestic political forces that challenged or deterred him. But, if the figure is less colossal, the achievement is no less remarkable.

Although an obvious continuation of the earlier volume, this work does have an independent existence. But the nonspecialist should read one of the other general studies of the congress as background.

That will not only enhance understanding, but also heighten the appreciation for what Kraehe has accomplished by painstaking archival research, a thorough and sensitive reading of secondary works, and a clear if not always eloquent prose.

WILLIAM J. MCGILL

National Endowment for the Humanities

JOANNA RADZYNER. *Stanisław Madeyski, 1841–1910: Ein austro-polnischer Staatsmann im Spannungsfeld der Nationalitätenfrage in der Habsburgermonarchie*. (Studien zur Geschichte der Österreichisch-Ungarischen Monarchie, number 20.) Vienna: Österreichische Akademie der Wissenschaften. 1983. Pp. 350 S 644.

Joanna Radzyner has written an excellent biography of Madeyski, based on his hitherto unused personal papers and on a wide variety of other sources pertaining to the politics of both his home province of Galicia and the Habsburg monarchy as a whole. This enables her to give full weight both to Madeyski's career as an Austrian statesman and to his class and national concerns as a Polish nobleman. The thesis of the book, in fact, is that the former was in large part the outcome and reflection of the latter.

This thesis is in itself neither debatable nor surprising. What makes Radzyner's book interesting is its depiction of how in practice Madeyski expressed his Polish noble concerns in his role as Austrian statesman, above all through his persistent advocacy of cooperation between Austro-Poles and Austro-Germans. As early as the 1880s, at the time of the Slav-clerical "Iron Ring" coalition, he regarded the exclusion of the Germans from political power as a mistake from the Polish point of view. The Germans were the only nation, he thought, that could preserve the unity and strength of Austria, and the Poles above all needed Austria to be united and strong if Galicia was to be preserved as the sanctuary that they needed so badly following the failure of the 1863 uprising (p. 64). Many years later Madeyski played an important part as a member of the *Reichsgericht*, the highest court of Cisleithanian Austria, in bringing about the decision of 1904 denying the Czechs of Vienna and Lower Austria the right to public primary education in their own language—a decision that in effect ensured the eventual assimilation of the Czech minority by the German majority. The Poles, of course, were threatened in Galicia by the rise of Ruthenian nationalism, just as the Germans of Vienna and Lower Austria were by Czech immigration. So strongly did Madeyski believe in the need to maintain the privileged position of the "historic" nations within the traditional Austrian crownlands that in Lower Austria he was even

prepared to sacrifice a fellow Slav nation (and not a "historyless" one either) when its activities threatened the locally dominant nation (pp. 313–14).

The culmination of Madeyski's policy of alliance with the Germans was the Windischgrätz coalition of 1893–95, in which three *Reichsrat* parliamentary groups—the Polish Club, the German Liberals, and the aristocratic-clerical Hohenwart Club—attempted to govern Austria in alliance against new radical and social mass movements, notably, the Young Czechs, the Christian Socials, and the Social Democrats. This attempt, and its failure, was a critical episode in Austrian politics and a climactic one in Madeyski's career, for he was one of the makers of the coalition and, as minister of education, an important member of it. Unfortunately, it is precisely here that Radzyner's best source, the Madeyski papers, fails her. Her account does not add much to what is already known of Madeyski's role in the coalition, although it contains some interesting material on reactions in Galicia to the events in Vienna.

Radzyner's book is valuable not only as a political biography but also as a social history. Madeyski was an example of a characteristic nineteenth-century social type: a member of the lesser nobility who moved off the land and into the educated, and usually state-salaried, professions. The usual image of this process is that it was in some way "forced," a word that Radzyner herself uses ("gezwungen," p. 46, n. 5), that those nobles who entered the educated professions presumably did so reluctantly and felt themselves thereby declassed. But her own vivid portrayal of Madeyski's background and rise gives a very different impression: that of a self-made man grimly determined to get ahead, very much on the bourgeois model. All of the elements of such a type are there: the frugal home life; the strong mother; the intelligence and hard work rewarded with academic success; the occasional "pragmatic," if not exactly questionable, means used in order to land a good job; the sense of isolation from associates of more established social position and lesser talents; and even the unsuitable wife, acquired at a time when Madeyski did not know how high he was going to rise (pp. 46–47, 76, 145). Of course, Madeyski was unusually successful, but perhaps we ought to alter our picture of the nineteenth-century lesser nobility. Rather than being a class of ne'er-do-wells who had to be provided with jobs at state expense, perhaps they were a class of meritocrats whose uncertain social position gave them a strong inner drive to succeed.

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VINCENT ILARDI, editor. *Dispatches with Related Documents of Milanese Ambassadors in France and Burgundy, 1450-1483*. Volume 3, 11 March-29 June 1466. Translated by FRANK J. FATA. Dekalb: Northern Illinois University Press. 1981. Pp. lxx, 445. \$35.00.

After more than a decade, with a change of publisher and a new translator replacing the late Paul M. Kendall, Vincent Ilardi continues his monumental edition of dispatches to the Sforza government from the Milanese ambassadors resident in France or Burgundy from 1450 to 1483. As promised in the preface to volume 1, this third volume comprises the dispatches from France following the death of Francesco Sforza on March 8, 1466, to the end of June of that year, omitting the diplomatic correspondence from 1461 to early 1466 already available in the four-volume edition of Bernard de Mandrot published early in the century. This third volume follows, with minor changes, the scope and format of the earlier volumes in the series. Ilardi has edited the Italian text from transcriptions of dispatches prepared by Italian collaborators working in the State Archives of Milan, and Frank Fata has provided a readable, accurate, if sometimes very literal, English version on facing pages. The introduction on the historical situation in 1466 and the notes identifying personages, events, and places reflect Ilardi's masterful grasp of Renaissance diplomacy and his capacity for clear, concise exposition. Minor changes from earlier volumes include printing passages originally in cipher in italics (rather than small type) and numbering letters for each volume (instead of the entire series). There is a good working bibliography of manuscripts and printed works, a full index, and adequate maps. The volume is handsomely printed and sturdily bound—a pleasure to consult and to read.

Of the fifty-two dispatches edited here, thirty-eight originated in France and were written by Emanuele de Iacopi or G. P. Panigarola, confidant of Louis XI; fourteen are replies from Galeazzo Maria Sforza, the new duke of Milan, and his mother, the widowed Duchess Bianca Maria Visconti Sforza. Their contents reflect some of Louis XI's preoccupations that spring: the creation of marriage alliances, intrigues of the houses of Burgundy and Anjou, and royal policy toward Savoy, Venice, and Milan. Hence, at first glance, this edition mainly provides essential texts contributing to an understanding of French royal policy following the defeat of the League of the Public Weal and the relations of France in 1466 with its principal Italian ally, the Duchy of Milan. But the texts also contribute fresh evidence on the language and methods of diplomacy and statecraft of a major Italian power in the generation before Machiavelli. Not least among the multiple benefits of this competent and attrac-

tive edition is a further contribution to the usage of that vexing term *lo stato*.

BENJAMIN G. KOHL
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MARK HULLIUNG. *Citizen Machiavelli*. Princeton: Princeton University Press. 1983. Pp. xi, 299. \$22.50.

The dust jacket of Mark Hulliung's revisionist essay on Machiavelli presents the author's thesis even before the reader has opened the book. Displayed thereon are a lion and fox rampant, holding between them a rosette containing the Florentine lily. The book offers a lucid, if contentious, case for the idea that dominion and manipulation, geared to military and political expansion, are the controlling themes not only of Machiavelli's *Prince* but also of his entire oeuvre. This constellation of ideas, the author argues, is basic to Machiavelli's justification of the republic as his preferred form of government. It shapes his view of ancient Rome and Renaissance Florence alike. The political scientists and historians concerned with Machiavelli's post-Renaissance fortunes who have annexed him to the republican tradition, Hulliung insists, have erred by confusing humanism with humanitarianism. Thus, they have failed to come to grips with the aggressive, imperialistic direction of Machiavelli's republicanism. For their part, he charges, the Renaissance scholars who have explored Machiavelli's relationship to Florentine civic humanism and contemporary Italian history have also erred by domesticating Machiavelli and robbing him of his originality. Hulliung, a professor of politics, thinks that Machiavelli belongs not merely or mainly to history, but to the ages. In an argument not calculated to recommend itself to readers of the *AHR*, he asserts that the effort to understand Machiavelli in historical context has the effect of burying him, consigning him to the past rather than clarifying his continuing importance.

Hulliung thinks that both sets of scholars have tried to neutralize Machiavelli and to deprive him of his shock value. For his own part, in seeking to explain Machiavelli's perennial capacity to jolt his readers, the author argues that Machiavelli deliberately subverted the classical and humanist traditions in order to make his sources subserve his own conception of political greatness, which was to be achieved by republican imperialism through the agency of force and fraud. This process of subversion has two dimensions. It demands the substitution of a pagan ethics for a Christian one. It also requires the substitution of an epic ethos of conquest, now yoked to the republic, for the Roman-Stoic ethos of glory achieved through public service as found in Vergil, Livy, and, above all, Cicero, an ethos that the Florentine civic humanists embel-

lished in turn with moral and aesthetic trappings. Hulliung takes a leaf from the book of John H. Geerken and his thesis on Homeric *virtù* in Machiavelli and from the book of J. H. Hale and his description of the *Prince* as the bomb in the psalter. In anatomizing Machiavelli's subversive technique, he shows how the Florentine capitalized on his audience's familiarity with his sources precisely in order to stand the *studia humanitatis* on its head and to force from it a warrant for Machiavellism, not only in the *Prince* but also in the *Discourses* and the *Florentine History*.

The first and last chapters of the book summarize the author's thesis and arraign the scholars whom he seeks to place in the dock. In between are four chapters detailing Machiavelli's high-handed treatment of the Roman and Florentine republics and his manipulation of his Roman and civic humanist materials. Hulliung also includes a chapter on Machiavelli's letters and comedies in an unconvincing effort to read them as an expression of Machiavellism in *pantofole*, a form of sexual politics that he labels "republican love." Hulliung is not conversant enough with the tradition of comedy, farce, and fabliau up through the Renaissance to realize that his claims here are exaggerated. Renard the fox and Isengrim the wolf had been at their merry pranks for centuries, and the doublethink enabling untold members of the fraternity to preach domestic loyalty while enjoying extracurricular erotic conquests does not necessarily require or bespeak a Machiavellian outlook.

The most welcome contribution made by this book is found in the four central chapters. Hulliung establishes that Machiavelli was not a historicist or a realist but an ideologue, determined to impose his own agenda on the texts he cited and the events he observed. He pressed them into the Procrustean bed of his own theories and ignored the problematic counterevidence—hence Machiavelli's numerous inconsistencies, his strategic omissions, his tendentious definitions, and his attempts to dissolve real difficulties in the alembic of his epigrammatic wit. Hulliung's own crisp and aphoristic prose enables him to display to excellent effect both Machiavelli's style and his substance.

The weaknesses that remain stem from a revisionism so overenthusiastic that it leads to overstatement and oversimplification. The major case in point is Hulliung's treatment of Vergil, Livy, and Cicero as avowed Stoics, although each of these authors was deeply critical of Stoic ethics. If Machiavelli subverted Stoicism in order to promote a different version of pagan *virtù*, the process was less discontinuous with his Roman sources than Hulliung admits. If the classical political tradition was not monolithic, furthermore, neither was the tradition of political Christianity. Hulliung accepts at face value

Machiavelli's definition of Christian ethics as political passivity. Yet here too the Florentine deliberately omitted and distorted inconvenient ideas and facts in defense of his own predilections. The Christian tradition was replete with theoretical rationales for military and political activism and even for aggression. In fact as well as theory, pacifism, not the just war or the holy war, was the exception that proved the rule. In turning crusaders into Christian martyrs, Machiavelli perpetrated a tendentious and ironic inversion of a familiar tradition just as surely as he converted Roman republican worthies into crafty imperialists. These two strategies of subversion are correlative and equally sophisticated, and they merit equal attention. That they fail to receive it places Hulliung in the class of political scientists who have yet to note and to credit the impact of the canon law tradition in the making of the European political mind.

Finally, while seeking to render due homage to the expansionist theme, Hulliung does not acknowledge other scholars who have pointed to this ingredient in Machiavelli's assemblage of justifications for republics. At the same time, he underrates other justifications also prominent on Machiavelli's list, including economic growth, the rule of law, and the protection of private rights. Although annoyingly monocular or unbalanced in these respects, Hulliung has nonetheless written a stimulating and challenging book, which deserves to be taken seriously by anyone interested in Machiavelli. One hopes that the publisher will issue a paperback edition without delay.

MARCIA L. COLISH
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FRANK J. COPPA. *Pope Pius IX: Crusader in a Secular Age*. (Twayne's World Leaders Series, number 81.) Boston: Twayne. 1979. Pp. 266.

After a century of polemics, the reign of Pope Pius IX (1846–78) is now receiving the disinterested scholarly attention it deserves. Roger Aubert's history of the Catholic church during this era was an early breath of fresh air. The more recent, exhaustive work of Alberto Serafini and Giacomo Martina enjoyed the advantage of access to the Vatican Secret Archives. Frank J. Coppa's brief study (which includes an excellent critical bibliography) does not pretend to supercede the work done by these scholars. Indeed, it relies heavily on them, although the author also uses unpublished sources. Coppa's more modest aim is to provide "a broad yet scholarly survey of the life and times of Pius while objectively assessing his impact upon the *Risorgimento*, Italy, and the Church" (p. 9). Although the impact of Pius on the church is slighted, the author essentially realizes

his other goals and gives us the best introduction to this subject available in the English language. He has written a sympathetic yet critical account, avoiding the apologetic tone that disfigured the work of even scholars such as J. B. Bury and E. E. Y. Hales. It is all the more regrettable that the author's clear but undramatic prose will probably deny this book the wider popular audience it merits.

The portrait that emerges from Coppa's biography is now a familiar one. Pius sympathized with the cause of Italian nationalism but was never a liberal and never endorsed political policies that endangered the spiritual mission of the church and the papacy as he saw it. His flirtation with the Italian national cause during the first two years of his pontificate was the product of political naiveté and a sincere desire to help the Italian people. His later intransigence toward almost all things modern was the overreaction of an intensely religious priest who trusted God and His providence more than diplomatic maneuvering. For better or worse, Pius was "the chief architect of the modern Church prior to John XXIII and the Second Vatican Council" (p. 198).

Yet it is precisely the religious influence of Pius on his church that is inadequately treated in this book. The emphasis is on Pius's disastrous political policies, which were largely repudiated by his successors. Adequate attention is given also to doctrinal matters such as the Immaculate Conception of Mary and papal infallibility, but important religious activities, including the expansion of missionary work, the reforms of the liturgy, and the centralization of religious authority in the Vatican, are only briefly touched on. A pope who insisted on the primacy of religion over politics deserves a biography that achieves a better balance between the two.

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G. PROCACCI, editor. *Stato e classe operaia in Italia durante la prima guerra mondiale*. (Studi e Ricerche Storiche.) Milan: Franco Angeli. 1983. Pp. 340. L. 16,000.

As G. Procacci astutely observes in the introduction, scholars who have written about Italy during the Great War have concentrated on the political and military history of those years. With the exception of Luigi Einaudi in his classic work, *La condotta economica e gli effetti sociali della grande guerra* (1933), authors have tended to treat the economic and social effects of the war as part of either the process of industrialization or the long march toward fascism.

Only recently have scholars begun to examine the economic and social effects of the war as a discrete

subject. The publication of this collection of papers, the proceedings of a conference held in Rimini in late 1982, is an important contribution to our understanding of the war and its effects. The articles in this volume remind us that the effects of the war were extremely severe in Italy. Politically, for example, the Italians never rallied to the war effort the way other Europeans did. In Italy the question of intervention and war divided all classes so badly that the government had to rule without parliament in order to avoid debates that would reveal the severity of the divisions.

Economically and socially the war accelerated Italy's drive to become a modern industrial state. In turn, as a number of the contributors indicate, government intervention in all aspects of the society increased, class divisions sharpened, and the social compact constructed by Giovanni Giolitti collapsed. The articles by Luigi Tomassini, Bruno Bezza, and Simonetta Ortaggi on the mobilization of Italian industry for war production demonstrate how employers used the war to attack the working-class movement and how the state, in its desire to maintain social peace, disciplined the workers with regulations that violated every concept of civil liberty, thus repressing both the workers and their organizations.

The articles by Alessandro Canardo, Santo Peli, Paride Rugafori, and Duccio Bigazzi examine the changes that took place within the working class and the workplace during the war. Particularly good is the article by Peli, in which he raises a number of questions about the role of women, draft dodgers, and militants within the working class. The third series of articles, written by Vittorio Foa, Maria Luisa Pesante, Renato Monteleone, and Patrizia Dogliani, discusses the social effects of the war in Great Britain, Germany, Austria, and France. Although each article is brief, they all give some basis for comparison with the Italian situation and demonstrate the fragility of the social fabric in even the most highly developed states.

As in every collection of articles, some pieces are better than others. All of them, however, would have merited from careful editing. They are presented as delivered, so the style is often difficult for the reader. In addition, the most valuable contribution of this kind of book is often bibliographic. The editor (or the publisher) unfortunately chose not to include a bibliography, and, since many of the articles furnish almost no citations, the book is less valuable than it might have been. Still, the number of first-rate papers and the excellent introduction by Procacci make this book an important contribution to our understanding of the war and its effects on Italian society.

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GIOVANNI SPADOLINI. *Italia di minoranza: Lotta politica e cultura dal 1915 a oggi*. (Quaderni di Storia, number 63.) Florence: Felice Le Monnier. 1983. Pp. xv, 428. L. 20,000.

Dean of Italy's modern historians, author of more than twenty books, former editor of the *Corriere della Sera*, editor of the *Nuova Antologia* (Italy's oldest scholarly review), and the present Italian defense minister, Giovanni Spadolini entered politics at the urging of Republican party leader Ugo La Malfa, becoming the Italian republic's first non-Christian Democratic prime minister and the country's most popular party leader. As his latest book demonstrates, his interests in cultural and political history remain lively and his production enormous.

A history of political and cultural struggles in modern Italy, *Italia di minoranza* discusses the paramount role of maverick Marxists, nonconfessional Catholics, and unconventional democrats in the country's development. These "losers" challenged accepted modes of thinking—Fascist, Catholic, communist—at great personal cost and left an indelible mark on Italian culture, despite their failure to conquer government institutions or mass party structures. More important, Spadolini argues that they created a living democratic tradition that remains the robust guardian of Italian liberty against the enemies that once overwhelmed and still threaten it. An essential "third force" in Italian life and culture, the "minority" personages and ideas that Spadolini analyzes exist outside mass parties and movements yet strongly relate to the mind and spirit of the Italian people. Only organizational defects prevented these minority personages from coming to power.

This laic democracy's roots go back to the Risorgimento and Mazzini, the prophet of a democratic and united Italy. Risorgimento ideals created an antifascist tradition of lonely heroes struggling against dictatorship at home and abroad. These included Luigi Salvatorelli, Mussolini's opponent who was deprived of his livelihood but wrote the extraordinary *Pensiero e azione del Risorgimento* that profoundly affected the future Action party, in whose history Spadolini sees reflected the organizational fate of the "third force"; Piero Gobetti, the youngster the Fascists beat to death, but not before his writings and editorial activity shattered the Fascist-imposed cultural silence and made him "a moral force" in modern Italian history; Gaetano Salvemini, the socialist gadfly who went from Marx to Mazzini, discovering in the Risorgimento leader a historic apostle of social progress and a fount for his own antifascism. From these roots arose the antifascist "Mazzini Society" in the United States and "Giustizia e Libertà" in Europe.

Paradoxically, included in Spadolini's "minority Italy" is Giovanni Giolitti, a personality subjected to opposing historiographical interpretations and to whom Spadolini has devoted several books. Condemned by both the left and the right, Giolitti had to be resurrected by the American historian A. William Salamone. In a marvelous portrait entitled "Giolitti in His Slippers," Spadolini invokes the legacy of this calumniated statesman whose regime guaranteed even the excesses of Italian culture and who anticipated the difficult choices confronting a modern industrial society, such as Italy became after World War II. In this context Spadolini analyzes the democratic socialist experience. Defeated by the Italian Marxist-Leninist establishment, this current survives, along with the rest of "minority Italy," in the people's aspirations. Togliatti said, "Small parties, small ideas," but never was an epigram so wrong.

Consisting of fifty-eight chapters, each a small gem profiling people or movements, *Italia di minoranza* has a structure that is logical and appealing and writing that is crisp and incisive. For the author's breadth of knowledge, insights, and personal reminiscences, every Europeanist should read this book.

SPENCER M. DI SCALA
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MIRCEA MUŞAT and ION ARDELEANU. *De la statul Geto-Dac la statul Român unitar* [From the Geto-Dacian State to the Unitary Romanian State]. Bucharest: Editura Ştiinţifică şi Enciclopedică. 1983. Pp. 723. 77 L.

An interesting schism exists today within Romanian historical circles. One division is between those who support the traditional Daco-Roman-Romanian approach to the origins and evolution of the Romanian people (in the main, the academic professionals) and those who support an alternative theory that may be called "Thracianism," an approach that minimizes or discounts Roman influences and emphasizes the pre-Roman heritage of the Romanians (historians close to Romanian Communist party circles as well as a portion of the literary and cultural apparatus). This book is the first comprehensive attempt to present the Romanian past before 1921 as seen by the party group.

There are five major themes. The first is that Romanian history can best be seen as the story of an unbroken 2000-plus year state tradition, that is, from the Thracians down to the present. The second is that any treatment of the Romanian past must be "unitary," that is, a chronological, thematic, and geographical whole. The third is that the central thread of the development of the Romanian people is continuity, that is, the uninterrupted

progress of one and the same people from prehistoric times to the present. The fourth keynote is on the resistance of the Romanian people to hostile neighboring empires and the like, that is, their success in maintaining autonomy and independence while defending Western civilization. Fifth, the book sees Romanian history as a history of the masses, that is, anything of note is a product of the ineluctable forces of history acting upon and through the toiling masses.

These themes are of varying utility and edification. To present Romanian history as a continuous "state" tradition from pre-Roman times is bizarre, to say the least. The Thracianist exaggeration not only lacks sufficient evidence; it also weakens the substantial gains made by Romanian archaeologists and medievalists over the last century and seriously undermines the international credibility of Romanian historiography. Since Romania's classic prewar histories were all pointedly "histories of the Romanians" rather than of Romania per se, the unitary emphasis is hardly an innovation. In the present context, it is mainly a blow in the current battle for amalgamation of the party- and university-linked institutes of history (to the likely detriment of the latter). The continuity theme is one that saturates Romanian intellectual life, let alone historiography; its appearance here is no innovation, except for the stress on ethnic continuity across the millennia—another conjecture that cannot be corroborated or refuted. Least interesting of all is the hackneyed masses theme, an idea that has received continuous lip service since 1944 in Romania (and elsewhere). Its stress in the present work is an augur of no particular future benefit.

Mircea Mușat and Ion Ardeleanu are not as extreme on these questions as some of their colleagues, however, and clearly their interests lie in the fourth area, particularly the issues of the pre-World War I Romanian irredenta and the war itself. Here their book makes a number of significant contributions, especially in discussing matters usually swept under the carpet or glossed over. The detail (lengthy quotes, extensive statistics) in the chapter on "Romanian Provinces under Foreign Domination" has created a sensation in a Romanian reading public accustomed to only the most oblique references to such matters, since it involves not only Transylvania and the Dobruja, but also the Bucovina and Bessarabia—areas now under occupation by the Soviet Union.

Overall, what can be said of this book? Where it differs from the theses of Romania's academic historians, the weight of the evidence falls solidly on the side of the academics. In their frank discussions of nineteenth-century Bessarabia and Bucovina, the authors are courageous and helpful. They aspire to write "history as it was . . . to present the facts not

after the subjective wishes of men, not after the political needs of the moment, based on situational criteria, but as they occurred." The authors fall considerably short of this objective. Given the current cultural, political, and historiographical situation in Romania, it may be a long time before such desirable goals can become a reality.

PAUL E. MICHELSON
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DAVID BRITTON FUNDERBURK. *Politica Marii Britanii față de România, 1938–1940: Studiu asupra strategiei economice și politice* [The Policies of Great Britain toward Romania, 1938–1940: A Study of Economic and Political Strategies]. Bucharest: Științifică și Enciclopedică. 1983. Pp. 221. 23 L.

David Britton Funderburk, the current ambassador to Romania, completed his doctoral dissertation in 1974; nearly a decade later that study has appeared in Romania. Although most historians will continue to consult the English version on microfilm, it is refreshing to see the publication of this work in Romania for its positive effects on Romanian-American relations.

The late 1930s, the author argues, witnessed a dramatic change in British interest in southeastern Europe. During the early interwar years British diplomacy showed little concern toward Romania; as French influence and presence waned during the next decade and a half, however, London stepped in to fill the void. The choice was clear: bolster and support Romanian neutrality or allow Bucharest to slip into the German orbit, and with it Romanian oil, which British military planners concluded would counterbalance any potential British naval blockade of the Third Reich. Beginning in the spring of 1938, therefore, Britain shifted its tactics toward Romania by offering certain economic proposals that, although disappointing in their results, demonstrated London's unwillingness to concede southeastern Europe as it had conceded the central portion of the continent. After the Czechoslovakian debacle, Britain, now hoping to use Romania (along with Poland) as a bulwark against continued German advances, increased its efforts by issuing formal economic and military guarantees to Bucharest. Although the hoped-for threat of a possible two-front war did not deter Hitler from attacking Poland, the British program, Funderburk concludes, did allow Romania to preserve its neutrality longer than might have been expected, since, for a brief period of time, it gave Bucharest the necessary great-power support with which to counterbalance the pressure from Berlin.

This last assertion is perhaps overstated by the author. Britain was hardly in a position either militarily or geographically to assure Romania's inde-

pendence. The provisions of the Vienna Award could never have been seriously contested by London. Nevertheless, apart from this flaw in the author's argumentation, this is a solid diplomatic study. Its importance, however, may lie not in its efforts to understand British policy in the 1930s, but rather in its analogy to present Romanian policy. Funderburk stresses that Romanian officials in the period 1938–40 followed a traditional pattern of trying to balance the interests of the great powers so as to preserve as much independence as possible. The efforts by the current Romanian government to maintain a similar degree of independence has given Bucharest the image of a "maverick" in Eastern Europe.

Whether or not that subtle analogy was a principal factor in the publication of Ambassador Funderburk's work, it is always encouraging to witness the appearance of any book in Romanian devoted to the sensitive period of the 1930s. For such a study to have been the product of a nonnative, especially of an individual in a position of authority, is even more satisfying.

RICHARD FRUCHT

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JOHN LOUIS HONDROS. *Occupation and Resistance: The Greek Agony, 1941–44*. New York: Pella. 1983. Pp. 340. Cloth \$25.00, paper \$12.00.

More than its European counterparts, the Greek resistance movement in World War II precipitated a national split whose reverberations continue to this day. Patriotic opposition to foreign occupation grew side by side with, and was eventually overshadowed by, virulent factionalism and a vendetta mentality. The guerrilla armies that harassed the occupation authorities were led by political factions whose visions of postliberation Greece could not be reconciled, or so it appeared at the time. The ensuing power struggle in the mountains and around conference tables led to sporadic violence even before the German retreat; this violence erupted into rebellion in Athens from December 1944 to January 1945. Moreover, in many respects the full-scale civil war of 1946–49 represented the rekindling of fires that had been started during the occupation. Thus, the record of the Greek resistance was badly tarnished and buried under the rubble of the country's postwar crisis.

It is this record that John L. Hondros sets out to rediscover and set straight. His purpose is to examine the Greek resistance movement not in light of postwar developments and partisan hindsight but on the basis of the wartime German, British, and American diplomatic and military archives. The

result is a minutely documented and persuasive study.

The book's organization is essentially chronological. It begins with the imposition of the Metaxas dictatorship in 1936, which, by destroying the parliamentary process and turning King George II into a symbol of authoritarianism, set the stage for the suspicions and intransigence that followed. After a brief review of the Italian and German invasions, it provides a short but vivid description of the harshness of the enemy occupation, the character of the collaborationist regime, and the rise of a spontaneous and undirected resistance. It then offers a detailed picture of the highly politicized organized resistance, the government in exile, and British efforts to foster resistance and simultaneously prevent the communist-directed National Liberation Front (EAM) and its military arm, the National Popular Liberation Army (ELAS), from dominating the field and imposing its political program after liberation.

Hondros's main conclusions, stated or inferred, can be summarized as follows. Greek resistance had a considerable impact on German military plans and was therefore worth the suffering it caused. Of the leading guerrilla forces, the ELAS was much larger, more popular, and more active against the enemy. Its principal competitor, the Greek Democratic National Army (EDES), developed into an instrument of the British authorities, who sought to build it up as a counterforce to the ELAS in an effort to frustrate the EAM's political program. Although both had occasional contacts with the occupation authorities, the EDES exploited them more effectively. In August 1943 a serious effort to reconcile the differences between the two factions failed because of the obstructionism of the Greek king and British diplomatic authorities and the weakness of the Greek government in exile. By the spring of 1944 (at the time of the Lebanon conference) the Foreign Office and the Greek premier, George Papandreou, had convinced themselves that the EAM-ELAS forces intended to seize power and that only British military action could forestall a communist coup. On the other hand, the EAM and the communists represented a bold political movement bent on preventing the king's return and opposing the prewar bourgeois parties but acting realistically and flexibly, without plans to resort to armed force to achieve their aims. Hondros concludes that the December 1944 uprising was a "colossal error" for the EAM-ELAS because it gave its enemies the opportunity to defeat it militarily as well as politically.

On balance Hondros's sympathy appears to be on the side of the EAM-ELAS, but his account is remarkably free of judgment, allowing instead the documents to make the desired point. His occasional

rebutting of other authors is restrained and carefully substantiated. But, if his story is without real villains, there is no mistaking where he places much of the responsibility—on the Greek king, who resisted all efforts to prevent or at least delay his return, and on Churchill, who stuck by the Greek monarch and compelled Papandreou to have a showdown with the EAM-ELAS.

Virtually all these conclusions have been presented before, often with greater eloquence. What makes Hondros's work unique is the meticulous weaving together of the enormously rich archival materials he has consulted. The result is a most convincing account and a valuable contribution to the study of modern Greece and of British wartime diplomacy.

JOHN O. IATRIDES

Southern Connecticut State University

KLAUS HELLER. *Die Geld- und Kreditpolitik des russischen Reiches in der Zeit der Assignaten, 1768–1839/43*. (Quellen und Studien zur Geschichte des Östlichen Europa, number 19.) Wiesbaden: Franz Steiner. 1983. Pp. vi, 273. DM 60.

Klaus Heller examines the financial policies of the tsarist state in an effort to explain why Russian industry lagged behind that of Europe in the nineteenth century. In particular, he traces the history of the paper "assignat ruble" from its creation in 1768 to the replacement of the assignat by the silver-backed "credit ruble" in 1839–43. Major milestones along the way included the establishment of several banks that provided loans in assignats as well as the various infusions of capital that the government itself received from European bankers to fund its wars.

Heller concludes that the government employed "false or anachronistic instruments" (p. 4) in its monetary and credit policies. Among the inherent flaws of the tsarist credit system were the six-month limit on loans (despite the need for year-long credit between the annual fairs) and the extension of credit only to gentry and merchants of the first and second guilds. By borrowing much of the available capital itself, the state "crippled private initiative in all sectors of the economy" (p. 246).

As for monetary policy, the tsars showed little concern for the stability of the paper ruble. In wartime, the printing press flooded the economy with assignats, so that the rate of exchange (*kurs*) of the assignat ruble drifted ever downward against major European currencies. The government also sought to extract from industrial enterprises a maximal tax revenue. All of these policies served to perpetuate the disadvantages of backwardness.

Copious documentation supports the narrative, and the bibliography is exhaustive. Especially useful are six appendixes, which show the number of assignats in circulation each year; the annual *kurs* in Amsterdam, London, Hamburg, and Paris; domestic rates of exchange among assignats, copper rubles, and silver rubles; the increase in the state debt; the assets and liabilities of the Loan Bank (1817–59); and the deposits and discount activities of the Commercial Bank (1818–59).

Two shortcomings should be noted. First, the monograph considers the capital shortage as the primary cause of economic stagnation, paying little attention to such legal restrictions as the gentry's monopoly on the ownership of populated land. It also focuses exclusively on the policies of the state. Although Heller makes a strong case against Alexander Gerschenkron, who was inclined to see the tsarist bureaucracy as the initiator of economic development, the feebleness of Russian entrepreneurship can hardly be explained without an inquiry into the attitudes and behavior of the entrepreneurs themselves. Granted, the archival sources and secondary literature for the state's side of the relationship are more abundant than the meager records left by the Russian merchants. Still, the historian who follows the paper trail simply because it is there risks giving a one-sided account of one of the most complex questions in current Russian historiography—how capitalist institutions evolved under the tsarist regime.

THOMAS C. OWEN

Louisiana State University

ALFRED J. RIEBER. *Merchants and Entrepreneurs in Imperial Russia*. Chapel Hill: University of North Carolina Press. 1982. Pp. xxvi, 464. \$35.00.

For a long time many historians have agreed that the pattern of social development of tsarist Russia differed substantially from that of Western Europe. The agreement owes its widespread acceptance, however, not to close study of eighteenth- and nineteenth-century Russian society—such work is just beginning—but to assumptions derived from Russia's unique revolutionary experience. One major element in this view is inspired by a nonevent, namely, the absence of a successful liberal revolution. Since Russian liberals failed to establish a democratic state, one presumes that Russian society did not produce a "full-blown bourgeoisie" to sustain a powerful liberal movement. Alfred J. Rieber accepts this proposition in his study of that country's "merchants and entrepreneurs," seeking in the history of these business groups the explanation why liberalism failed in Russia.

His interpretation draws heavily on the published records of business organizations in Moscow and St. Petersburg and on the published and manuscript records of the activities of a small group of Moscow businessmen. This rich fund of information, hitherto untapped, lends itself to detailed analysis of the political and social attitudes of the great Moscow business families. The first six chapters, constituting almost two-thirds of the book, examine the evolution through the nineteenth century of the merchant estate and the emergence of an embryonic "bourgeoisie" in the form of a relatively small number of entrepreneurs. The last chapters trace the political response of these two groups to the tumultuous years of revolution and war leading to the collapse of the tsarist regime and of their own comfortable world.

The distinction made between "merchantry" and "entrepreneurs" represents an essential assumption in Rieber's argument. He considers the former to be a group distinguished by its social and political conservatism. He emphasizes repeatedly throughout his work the resistance of members of this traditional estate to risky economic innovations and to liberal political ideals. His characterization relies on a very selective choice of evidence, however. The observations of one aristocrat traveling through the Volga in the 1850s lead him to conclude that "suspicion of outside influences" explains the refusal of Volga grain merchants to use the new railroad; the possibility that these traders found the recently improved river-canal transportation system to Moscow and St. Petersburg more reliable and less costly presents an issue neither the observer nor Rieber cared to discuss.

The ranks of the merchantry experienced in the nineteenth century a dramatic flux of individual fortunes as economic conditions changed. For example, former peasants were in mid-century entering the merchant estate through successful trading and manufacturing ventures. Rieber, in noting this trend, observes that "in their pursuit of profits the new guild members from the trading peasantry behaved more like genuine merchants" than many old members of the estate. Yet he dismisses these signs of upward social mobility with the comment that "Russian urban society was repeasantized," reiterating the dubious argument of pervasive backwardness within the country's "underdeveloped urban society." His very definition of "merchant" precludes comprehensive analysis of social and economic change within the commercial and industrial sectors of Russia's urban population. In his study, the enterprising belong in a group apart.

These "entrepreneurs" constitute the real subject of this work. Their family histories are described in great detail, while their political ambitions and activities earn them a careful and sympathetic hearing

from the author. He presents detailed biographical information on the professional and political activities of Moscow's leading business families, divided into a "first generation" active in the 1860s and 1870s and a "second generation" prominent after the revolution of 1905. He finds that they evolved an "oligarchic style of politics," cut off from the less prestigious and successful business groups in central Russia and unable to form ties with the non-Russian entrepreneurs operating around the empire's periphery. A second, more general reason for the stunted social support of liberalism is found, in Rieber's opinion, in what he terms the "general disintegration of all social forces in Russia" in the early years of this century. This apocalyptic view of Russian prerevolutionary society leads him to conclude that Moscow's entrepreneurs could not possibly "break out of the social isolation that weakened their claim to represent the emerging elite of industrializing Russia in an era of mass politics." In other words, social "fragmentation" prevented the emergence of a Russian "bourgeoisie," thereby undermining liberalism and paving the way for Bolshevik dictatorship to follow the collapse of the autocracy.

A brief review cannot do justice to this complex and provocative argument. Rieber's study represents an important contribution to the debate over the social origins of the Russian Revolution and for this reason deserves careful reading. In this reviewer's judgment, however, it is less satisfactory as a contribution to the history of Russian society. Its data on the Russian business groups do not constitute the bases for a real collective biography, even of the Moscow merchantry. Its criteria for analysis are seriously flawed by questionable assumptions of Russian social and economic backwardness. The real history of the Russian middle class has yet to be written.

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FRED V. CARSTENSEN. *American Enterprise in Foreign Markets: Studies of Singer and International Harvester in Imperial Russia*. Chapel Hill: University of North Carolina Press. 1984. Pp. vi, 289. \$29.95.

"Underdevelopment" and "modernization" are terms used in economic history that often blind the researcher to the real matter of history: men and women doing certain things at particular times in a specific location for definable, if not rational, purposes. Fred V. Carstensen has not suffered that fate. He has written an informative, at times intriguing, and always human organizational history of management procedures, labor relations, and marketing within two multinationals, Singer Manufacturing

Company and International Harvester Company, operating in Russia during the last half-century of tsarism. This is primarily a case study of the development of an effective marketing strategy and organization that could tap latent Russian demand. Much of the virtue of this work emerges from its very narrowness, for Carstensen is engaged in a microanalytic test of what he describes as the standard characterization of late imperial economic development. According to that model, attributed by Carstensen primarily to Alexander Gerschenkron, "Much of Russia's backwardness . . . [resulted from] the venality of government bureaucrats, the 'disastrously low' levels of commercial honesty among Russian firms, and the poverty of the domestic market. In Gerschenkron's view, the state had to substitute its own initiative, energy and resources for the market in order for the industrialization process to proceed rapidly" (p. 6).

Carstensen bases this study on records contained in the archives of Singer and International Harvester, whose subsidiaries were the two largest commercial industrial enterprises in imperial Russia. He argues that foreign participation arose from opportunities intrinsic to the Russian market and that the two firms began in Russia with purely commercial ventures that turned to domestic manufacturing only to protect or further to develop established Russian markets. In this process, Russian officialdom revealed an "indifference or insensitivity . . . to general economic development" (p. 7). The success of these multinationals was built on abundant foreign capital, both operating and human.

Carstensen's main thesis is that "the Russian economy may have been more constrained by poor input markets [available capital and thus credit] and weak infrastructure [domestic entrepreneurial-managerial talent and marketing mechanisms] than by weak demand" (p. 101). Latent demand was there, but it was prevented from articulating itself by the absence of money and managerial resources on which to operate domestic business. Carstensen proves that a multinational, using its own extensive capital resources, could loan individuals one or two hundred rubles and that purchasers of consumer durables would accept that credit.

The problem, however, for genuine Russian industrial development, as Theodore Von Laue has demonstrated, was how to obtain the capital to build a Russian industrial and transportation infrastructure before Russia was colonized or partitioned, as China already had been by the West. True, the consumer market does appear broader and deeper, "more robust than many scholarly studies have recognized" (p. 96). But exactly how this market sphere fits into the problem of Russian modernization requires, and Carstensen calls for, additional study. Carstensen has done the discipline service by

bringing the question of the market to the fore. As he states, the purpose of such a case study is to raise new questions, such as the impact of new industrial products on people's livelihood and resource use; the relative weight of rural versus urban demand; and the retarding influence that multinationals, through their ability to provide substitutes for inadequate capital, credit institutions, distribution networks, and human capital, had on the emergence of domestic commercial-industrial classes and on the growth of pressure on the government to reform the domestic infrastructure. This he has done well.

HUGH D. HUDSON, JR.
Georgia State University

V. IA. LAVERYCHEV. *Gosudarstvo i monopolii v dorevoliutsionnoi Rossii: Problemy vmeshatel'stva absolutistskogo gosudarstva v ekonomicheskuiu zhizn' i vozdeistviia kapitalisticheskikh monopolii na gosudarstvennyi apparat* [The State and Monopolies in Prerevolutionary Russia: Problems of the Intervention of the Absolutist State in Economic Life and of the Influence of Capitalist Monopolies on the State Machinery]. Moscow: Mysl'. 1982. Pp. 198. 1 r. 20 k.

Opening with the obligatory recitation of verses from Lenin, Marx, and Engels, V. Ia. Laverychev embarks on a spirited examination of the relationship between government policy, government bureaucracy, and monopoly capital (that is, leading commercial and industrial interests) in prerevolutionary Russia, particularly during the period 1900–14. This study—Laverychev's fourth book on late imperial business, labor, social, and economic history—necessarily conforms to the Soviet historiographic tradition (expectation), which seeks to identify, in a way consistent with dialectical materialism, the patterns in tsarist economic life that led to the revolutions of 1917. But, within that framework, he subtly and effectively changes the structure of the analysis, moving the government from a position of uniform alliance with capitalist forces to one of frequent opposition and tension, with occasional instances of slow, begrudging cooperation. Such a pattern of interaction, even when it brought government policy and conduct into alliance with capitalist interests, pulled these interests within the governmental orbit, a process that prevented the "natural" maturation of a bourgeois-capitalist society. It was this divergence from Western patterns of development that created the conditions that led to the upheaval of 1917.

Laverychev follows a typically Soviet topical approach, organized in just three chapters. First, he assesses the development of monopoly capital at the opening of the twentieth century, tracing the emergence of various cartel structures (without offering

persuasive economic evidence or argument that such institutions were effective) and the growing influence of finance capital. He then discusses the efforts within the government to widen its direct role in the economy, the pattern of increasing government regulatory intervention—intervention largely for the benefit of the landlord, gentry class—and the opposition of the commercial and industrial interests to both forms of government expansion. The final chapter examines the emergence of government-monopoly capital cooperation and pays particular attention to regulation of sugar production, the pattern of railroad equipment orders, the impact of a maritime construction program, and the shared hostility to the labor movement. Here Laverychev emphasizes the merging of state and private interests into a distinctive and expanding pattern of state-controlled monopoly capital hostile to market competition.

Although Laverychev embeds this powerful and persuasive analysis in a plentitude of economic and business data, it is clear that neither the sugar industry nor railroad and maritime orders were of sufficient size to touch more than a small segment of private economic interests. Indeed, the broader argument—that the government's direct role in the economy was expanding—is buttressed neither by good comparative data in the book nor by Western scholarship. But the ultimate importance of Laverychev's argument does not rest on economic quantities but on his assessment of the political economy of the tsarist regime. It was a government deeply distrustful of private initiative and private organization. In both specific policies and general ideological perspective, it was hostile to a market environment and the attendant institutional foundation of the rule of law and due process. This is an effective rebuttal to Alexander Gerschenkron's well-known typological "backwardness" framework, which sought to place the late imperial experience firmly within the continuities of Western economic and political evolution. Although both more informative footnotes (Laverychev gives only archival file numbers, without identifying either the archival collection or the specific source document) and an explicit dialogue with Western scholarship (he cites none) would enhance this study, it is an important book, revealing a perceptive and critical approach to understanding the patterns of late imperial economic and political life.

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GREGORY L. FREEZE. *The Parish Clergy in Nineteenth-Century Russia: Crisis, Reform, Counter-Reform*. Princeton: Princeton University Press. 1983. Pp. xxxii, 507. \$50.00.

The Parish Clergy in Nineteenth-Century Russia is the most important study of modern Russian Orthodoxy since George Florovsky's *Ways of Russian Theology* appeared in 1937. Together with Gregory L. Freeze's earlier *Russian Levites*, an examination of Russia's Orthodox clergy in the eighteenth century, it is the only comprehensive account of the parish clergy in imperial Russia. Moreover, the work is built with the most extensive use of archival sources ever assembled by a Western scholar on Russian history. Combined with a broad reading of other primary and secondary literature, Freeze's archival work is placed in the wider European context of clerical problems and developments. The result is pioneering research that challenges traditional interpretations about modern Russian Orthodoxy (among many others, the prevailing view of Konstantin Pobedonostsev as the all-powerful ruler of the Russian church from 1880 to 1905).

Freeze describes a hereditary social estate or caste (*soslovie*) effectively isolated by its Westernization from the unlettered *narod* and by its poverty and low status from the remainder of educated Russian society. To overcome this isolation and make the church a greater moral force, leaders in both church and state continually tried to reform the clergy, concentrating first on parish service, education, and administration and eventually on the clergy's caste structure. Despite some important changes in the late 1860s that destroyed the hereditary character of parish service, the secular, or married, clergy met the revolutionary twentieth century divided, embittered, and impoverished—a heartbreaking outcome, considering the continuous efforts throughout the nineteenth century to reform and improve the clergy's position. Central to the failure was the tragic configuration of opposing interests. The state wanted a reform that would advance its interests, especially by strengthening control in the empire's western—Catholic, Uniate, and Lutheran—provinces and by enhancing its power over the church's administration at the expense of the monastic hierarchy. The bishops sought reforms that would reclaim their authority in the parishes, while fending off aggressive and often boorish state-appointed overprocurators (as well as frequent clerical criticism that the bishops cared little for the clergy's welfare and needs). The parish clergy wanted to raise their status economically and socially, prerequisites, they believed, to effective pastoral action. The laity, deeply resenting Western-inspired innovations that they neither understood nor wanted, desired clergy who could perform the traditional rites without adding financial burdens.

Freeze's study of the parish clergy—or, more accurately, his study of attempts to reform it—offers several challenges. Scholars must now provide a comparable study of Russian monasticism in order

to determine if the church might have had other ways of exerting influence on the laity than through the parish clergy. Russian literature suggests such a possibility. Greater attention must be given to popular religious life in order to understand the complex ways in which Russia's lower classes changed or developed their views during the imperial era. The picture we now have is too static. The history of the Orthodox clergy in imperial times must be linked with the Soviet experience, for some of the problems analyzed in this study found their apocalyptic resolution in the 1930s. How the contemporary church has dealt with the clerical weakness inherited with the revolution is still a largely unknown subject. All in all, Freeze's study is a *tour de force*. Students of Russia, especially those who probe its religious life, are deeply in his debt.

ROBERT L. NICHOLS
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E. A. DUDZINSKAIA. *Slavianofily v obshchestvennoi bor'be* [Slavophiles in the Social Struggle]. Moscow: Mysl'. 1983. Pp. 270. 1 r. 70 k.

This brief survey of Slavophile thought and practice caps some thirty years of reconsideration by Soviet historians of the place of Slavophilism in Russia's social evolution. E. A. Dudzinskaia's thesis, which is borrowed from Lenin, is that, far from being reactionary, Slavophilism was, like Westernism, a variant of progressive bourgeois gentry liberalism. Slavophile ideology, she maintains, was the product of its exponents' experiences as estate owners. Convinced that serfdom was a hindrance to profitability, the Slavophiles emerged as the self-conscious representatives of the true interests of the gentry during the emancipation debates; they advocated the introduction of capitalist relations into Russian agriculture while seeking social stability and labor continuity through the defense of autocracy and the communal patterns of village life. The Slavophiles' emphasis on national self-consciousness served in Russia, as did nationalism elsewhere in Europe, to pave the way for bourgeois development.

Among chapters on the ideological origins of the emancipation reforms, the Slavophiles' view of the peasant commune, their role in the provincial committees on emancipation and on the editing commissions, and their relations with Herzen and Chernyshevskii, the most novel is a detailed chapter on their theory and practice of estate management. Although highly informative, this pivotal chapter does not in the end succeed. As the author shows, the Slavophiles were unquestionably in the forefront of agricultural innovation in Russia, but it does not follow that the entire elaborate edifice of Slavophilism ultimately arose from their desire fur-

ther to improve their estates. To anyone familiar with the scope and depth of Slavophile thought the simple materialist account of their motivation rings resoundingly false.

The mandatory reductionism of Soviet historical science mars what in many ways is a valuable piece of research. In Soviet parlance, the definition of bourgeois is as broad as is required to make the data match the materialist framework. A liberal in the nineteenth century, it appears, is anyone who wittingly or unwittingly, willingly or unwillingly advocates or participates in furthering "progress," unless, of course, "progress" is immoderate, at which juncture he becomes a revolutionary democrat like Herzen or Chernyshevskii.

None of this assists us in understanding Slavophilism. The real contribution of Dudzinskaia's work lies in its detailed elaboration of the practical side of Slavophilism and Slavophile activism. Although there is little new in the book, it nevertheless comes as a timely counterweight to the growing trend, initiated by A. Walicki, to concentrate on the utopian element of Slavophile thought. The Slavophiles were neither reactionaries nor liberals, neither utopians nor pragmatists. In time, they will find their place within the history of Russian conservatism, which is yet to be written.

E. WAYNE DOWLER
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HANS HECKER. *Russische Universalgeschichtsschreibung: Von den "Vierziger Jahren" des 19. Jahrhunderts bis zur sowjetischen "Weltgeschichte," 1955-1965*. (Studien zur Modernen Geschichte, number 29.) Munich: R. Oldenbourg. 1983. Pp. xvi, 376.

This book provides detailed description of the life and work of academic historians who, in the nineteenth and twentieth centuries, lived and worked in Russia and whose scholarly careers, for the most part, were devoted to the study of Western Europe. The author shows that in Russia "universal" history meant, and continues to mean, the history of Western Europe. He rightly points out that most previous studies of Russian historical work have paid little attention to those whose subject was non-Russian history. Hans Hecker's goal is not so much to fill a gap in the literature as to argue the importance of two aspects of Russian study of Western Europe. First is the question of what Russians made of Western history. Nearly all Russian historians of the West became to some degree members of the opposition intelligentsia. They opposed not only the autocratic system of the tsars but also the radical extremists, who were the most violent opponents of the tsarist regime. The Russian professor of univer-

sal history became, consciously and conscientiously, a "political" professor who tried to foster in Russia the development of the values he found in the liberal West, especially the rule of law and respect for individual rights. Hecker makes clear that the "political" Russian professor, while morally committed to a set of political and social positions, by at least the 1870s met the standards of world—that is, French and German—professional scholarship. Hecker's second question is whether the 1917 revolution marked a break in or saw continuity in Russian study of the West. He finds much continuity in some senses, albeit forced into much narrower paths and with more important issues closed on principle to serious investigation than was true in tsarist times.

Part of the book's clarity results from its organization. The first chapter describes the institutions in which historians worked and that conditioned much of what historians could do, while the second describes the understanding of history that was the legacy of the N. M. Karamzin–M. P. Pogodin generation and the common possession of university-trained men who came of age in the 1840s. The next four chapters describe in turn the work of what Hecker calls four "generations" of Russian historians—the first, of T. N. Granovskii, the last, of M. N. Pokrovskii. The next four chapters are case studies of what Russian scholarship made of four particularly important topics (the Renaissance in Italy, the Reformation in Germany, the English seventeenth-century revolution, and the French Revolution), focusing on their meaning for Russia, not on their contributions to the scholarship of the topics themselves. A brief conclusion sums up Hecker's main points.

The main points established by Hecker are not surprising and require no major revision of our understanding of the development of modern Russia. Some sections of the book are thin—for example, the description of the universities, which is not more helpful than many textbook summaries or encyclopedia articles, relies excessively on the work of pre-1917 liberals such as I. N. Borozdin. Given Granovskii's importance, moreover, it is unfortunate that Hecker made no use of work on Granovskii completed over the past decade or so by American scholars, in particular, by Nicholas Racheotes and Priscilla Roosevelt, whose study (in *Forschungen zur Osteuropäischen Geschichte* [1981]), Hecker notes, appeared after his own work was complete. Nonetheless, Hecker's thorough command of the printed sources and secondary literature for most aspects of his subject and the rich detail he provides on the life's work of many historians make a most useful contribution. His main points have been broadly, and somewhat vaguely, sketched be-

fore. With detail and clarity, Hecker convincingly demonstrates their soundness.

JAMES T. FLYNN
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LINDA HARRIET EDMONDSON. *Feminism in Russia, 1900–17*. Stanford: Stanford University Press. 1984. Pp. xi, 197. \$25.00.

This study is the first to examine in depth the brief, sad history of Russian feminism in the twentieth century. It does so very satisfactorily and will prove valuable both to scholars studying Russian history and to those interested in the development of feminism.

Linda Harriet Edmondson begins with a survey of the nineteenth-century origins of the woman's movement in Russia. She then concentrates on the period from 1905 to 1914, examining the feminists' response to the upheavals of 1905, their plunge into despair and disorganization in the "years of reaction" (1907–11), and their rejuvenation in 1912. The period from 1912 through 1914 held genuine accomplishments, among them a significant rewriting of the marriage law. But the promise of these years for feminism, as for other reforms in Russia, was destroyed by World War I.

There are some weaknesses in Edmondson's narrative. One could wish for fuller portraits of the leaders of Russian feminism and for an analysis of their origins and experiences that suggests the reasons why these women chose to become feminists while so many others of their class did not. A more thorough discussion of the causes of the emergence of feminism in Russia and an identification of those aspects that were distinctively Russian would be helpful. Finally, Edmondson might have permitted herself a ruminative comparison of Russian feminism with feminist movements elsewhere. These matters are all discussed in passing; the book would have been stronger had they been developed at greater length.

But the strengths of this study far outweigh its weaknesses. Edmondson's judgment is judicious, her tone balanced and detached, though sympathetic. She writes clearly; the reader is never confused amid the labyrinth of groups and individuals. The story Edmondson tells is a gloomy one: Russian feminism faced insuperable obstacles, not least among them the fainthearted support it received from the intelligentsia. Despite their famous commitment to the woman question, Russian socialists and liberals alike were always willing to postpone their advocacy of feminism, if they judged that it would be politically advantageous to do so. Edmondson makes the telling point that only when power lay beyond its grasp was the Kadet majority, repre-

sented by Pavel Miliukov, willing to endorse women's suffrage. Edmondson also demonstrates that the feminists themselves were often divided and uncertain, sometimes dissipating their energies in internecine squabbling. The greatest cause of their ultimate failure, however, was neither the merely lukewarm support of the intelligentsia nor the feminists' own political shortcomings but the realities of Russia, which doomed reformers first to tsarist oppression and later to irrelevance in revolution.

BARBARA EVANS CLEMENTS
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REX A. WADE. *Red Guards and Workers' Militias in the Russian Revolution*. Stanford: Stanford University Press. 1984. Pp. vi, 391. \$32.50.

In this meticulously researched and clearly written study, Rex A. Wade examines the role of workers' militias and Red Guards in the Russian Revolution of 1917. His focus is chiefly on Petrograd, the epicenter of the revolution, but he analyzes the creation and activities of workers' armed bands in towns and industrial settlements throughout Russia, with particular attention to Saratov and Kharkov. His study is well organized, his judgments are balanced, and evidence is deployed judiciously. The tale he tells is a lively one and adds a vital piece to the jigsaw, being rapidly assembled, of the history of the revolution "from below."

During the February Revolution workers' militias were established on a local, factory basis to protect factory property and to uphold law and order in the working-class districts. They arose spontaneously, without the intervention of political parties, though inspired in part by the socialist ideal of a citizenry in arms. From the first, they clashed with the Petrograd City Duma, which, under the auspices of the Provisional Government, set about creating an all-class, nonpartisan police force subject to the official authorities. The eight months between February and October saw abortive attempts by the authorities, backed by the moderate socialists on the Soviet Executive Committee, to disband the workers' militias. The latter, however, were adamant that they would not submit to any nonproletarian body, and so, in each locality of the capital, workers' militias competed with the new civil militia in a microcosm of "dual power."

As the unpopularity of the government deepened, more aggressive, politicized formations were created—the Red Guards—committed to struggling against counterrevolution and to defending the gains made by workers since March. These Red Guards were backed by the factory committees and more militant city-district soviets but were independent of party control and had only passive

support from the Bolsheviks. In the teeth of fierce opposition from the Soviet leadership, the development of the Red Guards was slow, and the failure of the July Days set them back considerably (although Wade tends to minimize this). The real growth of a coordinated network of Red Guards came after the Kornilov putsch at the end of August. The sudden epiphany of ruling-class revenge spurred workers to seek out weapons, to train in their use, and to finish each day's work with military drill in the factory yard. By mid-October, Wade reckons, there were nearly two hundred thousand Red Guards throughout the country. He thus reverses the recent tendency of both Soviet and Western historians to scale down the number of Red Guards, though he blurs any distinction between the latter and the more apolitical militias. Again, swimming against the historiographical tide, he suggests that, in spite of ubiquitous complaints of arms shortages, weapons were in plentiful supply.

In October the Bolsheviks could rely on a powerful network of armed bands, consisting in the main of young workers who, though by no means solidly Bolshevik, were determined to establish soviet power. Wade argues that the Red Guards played a more active role in the insurrection than is usually assumed, suggesting that it was their ability to overawe opponents, rather than their doubtful military skill, that was crucial. Nevertheless, within a few months the new Bolshevik government had concluded that the Red Guards were unsuitable as the core of a regular army. Their spontaneous and local character and their paucity of numbers seemed incompatible with the perceived need for a centralized, disciplined, and efficient army. By the spring of 1918 the Red Guards had either dwindled away or been absorbed into Trotsky's new Red Army. They were thus the first of the popular organizations born of the 1917 revolution to fall victim to a combination of circumstantial exigencies and the Bolshevik penchant for centralism.

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GAVRIEL D. RA'ANAN. *International Policy Formation in the USSR: Factional "Debates" during the Zhdanovshchina*. Foreword by ROBERT CONQUEST. Hamden, Conn.: Archon. 1983. Pp. xi, 248. \$29.50.

This volume by the late Gavriel D. Ra'anan deals with one of the most intriguing historical problems of the mid-twentieth century, the Stalin-Tito controversy. It proposes that, in the postwar years, Stalin was periodically ill and in any case hesitant about policy; he therefore gave very considerable political leeway to factions within his regime. As a result, the grouping around Andrei Zhdanov gained a distinc-

tive ascendancy. Zhdanov encouraged the ambitions in Eastern Europe of Tito, with whom he had links dating from before the war. Tito thereupon in 1946–47 indulged in egregiously independent political initiatives; these in the end awakened Stalin's indignation and contributed to the collapse of Zhdanov's mansion in 1948–49.

Ra'anana not only reviews in detail the older evidence about these events but also adds material from the recently published memoirs of Antonov-Ovseenko, N. S. Patolichev, and Eduard Kardelj and from interviews with Soviet emigres (notably, Mark Kuchment, Alexander Nekrich, and Aaron Katzenelinboigen). But his real contribution to our understanding lies in the facility of his terminology. One may generalize: most earlier historians of the postwar period have been shy of being labeled "Kremlinologists" and have consequently tended to downplay the importance of the factions. The result has been accrued helplessness: because we did not discuss the factions, our tools for understanding them remained weak, while our understanding of other aspects of the period grew strong, and this relative weakness increased our shyness. Ra'anana does not fear the "Kremlinological" label. He lucidly characterizes the Stalinist factions as "feudal chieftainships"—Mafia-like alliances forged by party leaders as they rose to power. He distinguishes them, convincingly, from the "issue-oriented" and "bureaucratic-institutional" groups described in most literature about totalitarianism. His concepts will make it easy for historians to take the factions seriously and to acknowledge that, despite Stalin's dictatorship, these groupings were less instruments of Stalin's will than autonomously functioning sources of Soviet domestic and foreign policy.

This book can be recommended to all students of the end-of-the-war era with the caveat Robert Conquest inserts in his foreword: that one cannot agree with all of Ra'anana's interpretations. Conquest takes exception to some allegiances Ra'anana posits for specific personalities, but there are more important flaws. In his concentration on Zhdanov and Tito, for example, Ra'anana virtually omits from consideration the competing factions of A. S. Shcherbakov, G. M. Malenkov, and others. He also omits the evidence about the "bureaucratic institutional" developments that Werner Hahn has recently and elegantly described in his *Postwar Soviet Politics* (1982). Ra'anana surely exaggerates with his suggestion that Zhdanov's hurt ego, not Stalin's, led to the displacement of Marshal Zhukov in 1946; and, in my opinion, there is sheer wrongheadedness in his dissociation of Zhdanov from the "cosmopolitan" pro-Israeli Soviet policies of 1947–48. All told, however, this study is so well equipped with scholarly citations that any reader can discover the misinterpretations for himself.

Ra'anana has done historians a real service by creating so natural a terminology for discussing his subject and by writing so enjoyable a book.

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ROY MEDVEDEV. *Khrushchev*. Translated by BRIAN PEARCE. Reprint. New York: Anchor. 1984. Pp. x, 292. \$9.95.

ROY MEDVEDEV. *All Stalin's Men*. Translated by HAROLD SHUKMAN. Reprint. New York: Anchor. 1984. Pp. x, 184. \$14.95.

Roy Medvedev, as something of a one-man historical institute, continues his remarkable career in the Soviet Union. A committed "Marxist-Leninist" with impeccable anti-Stalinist credentials, he has so far escaped the fate of more celebrated dissenters, notably Alexander Solzhenitsyn and Andrei Sakharov. These two volumes under review—his historical corpus remains taboo in his homeland, circulating only in *samizdat* (that is, underground) format—seem likely to erode still further his precarious standing as a barely tolerated political eccentric. *All Stalin's Men* embellishes the devastating "exposé" of the great tyrant presented in *Let History Judge* (1972), his first venture into historical scholarship, and provides a unique repository from which Western specialists and biographers have freely borrowed. *Khrushchev*, though ostensibly a different subject, is in part a logical extension of the Stalin project. Both books are lightly and sporadically documented with reference notes but contain no bibliographies. There are, in addition, glossaries and biographical sketches of Communist leaders that are useful to general readers. The author does not appear to be well versed in the variety and richness of Western literature on the Stalin and Khrushchev eras, although he occasionally cites the best-known works and even mentions arcane emigre sources. But Medvedev's forte is that of the "insider" conveying information, presumably based on oral testimony, that is virtually inaccessible to foreigners. Unfortunately, he is seldom inclined to indulge his audience on this point, perhaps for good reason, yet it would be helpful to know which material is derived from high-level rumor and gossip and which is obtained from reliable witnesses.

All Stalin's Men (an ambiguous title) is a biographical study of six prominent members of the Communist party's ruling organ, the Politburo (later Presidium), whose careers continued well beyond the death of their patron. Among those eliminated by these criteria are such powerful figures as Sergei Kirov, the Leningrad party boss, who may have been assassinated at Stalin's behest in 1934; Andrei

Zhdanov, seemingly the heir apparent, whose demise in 1948 was not above suspicion; and Lavrenti Beria, the sinister police chief executed in 1953 by Stalin's successors. Indeed, at this writing three (Vyacheslav Molotov, Lazar Kaganovich, and Georgi Malenkov) are still alive, though long since relegated to obscure retirement. The others are Klementi Voroshilov, Anastas Mikoyan, and Mikhail Suslov. That all six were to some extent ruthless and unsavory products of the Stalin system would be a safe enough generalization, but the lurid subtitle on the book jacket, "Six Who Carried Out the Bloody Policies" (not repeated on the title page) is not borne out by the text. One supposes that the author would object to such hyperbole as a flagrant example of capitalist profit seeking. The biographies are brief, informative, and compelling. A rigorous search of Western sources might yield a richer vein of conventional detail—schools attended, positions held, and honors received—but only Medvedev is in a position to provide those political nuances that transcend formal biography. A modest and useful book, it adds another layer to the slow accretion of Stalinist lore and furnishes a backdrop to the Khrushchev regime.

The Khrushchev biography is an ambitious undertaking and rests to some degree on an earlier work written with his twin brother, Zhores Medvedev, *Khrushchev: The Years in Power* (1975). Biographies and other specialized studies on the Khrushchev years are not in short supply. Few, however, are satisfactory to academicians and other serious students of the period. Now that the topic has lost its contemporary flavor, it should be possible to reassess Stalin's chief "heir" with more detachment and insight. The Soviets, not unexpectedly, have declared the subject off limits, and it is a credit to Medvedev's fortitude and intellectual honesty that he has persevered in the face of possible reprisal. This is a sympathetic appraisal but by no means "authorized," despite the assistance of Khrushchev's family, especially for the retirement years. At times it is critical, even severely so, and demonstrates no hesitation in discussing Khrushchev's blunders and shortcomings, the most egregious of which involved the faltering economy. Medvedev is well informed on agricultural matters but for unexplained reasons has chosen to omit or truncate much of the incisive material on the ill-fated agrarian "reforms" contained in the earlier version of this work, particularly information on the virgin- and fallow-lands program and the precipitate dissolution of the machine-tractor stations in 1958. Curiously neglected also is the notorious case of Trofim Lysenko, the pseudo-biologist, whose career continued to flourish after Stalin's death.

Khrushchev's involvement with Stalin and the Stalin legacy, if not the core of the book, should

attract the most attention. There are no major revelations and few surprises on the Stalin era itself. Regrettably, these earlier years of Khrushchev's life are given short shrift, and Western biographers have furnished more detailed, though not necessarily superior, accounts. The vexed question of how far Khrushchev compromised himself during the political terror of the 1930s is disposed of somewhat perfunctorily. The conclusion that he bore moral responsibility without actually becoming an accomplice, though unexceptionable, could have furnished a partial explanation—namely, a guilty conscience—for his resolute determination to shatter the Stalin cult in 1956. Medvedev properly emphasizes Khrushchev's political courage and presents the most convincing version extant of the circumstances attending the famous "secret speech." The same may be said of the events surrounding Khrushchev's fall from power in 1964.

On most aspects of Soviet foreign policy Medvedev lacks the easy familiarity so evident in his recital of domestic affairs. At times his narrative becomes labored and surfeited with trivia, and, except for Soviet relations with other Communist countries, his confidential sources appear to be fewer and less knowledgeable. On occasion his patriotism (and Marxist ideology) rises to the surface, making it more understandable why his reputation as a bona fide dissenter has been questioned by some anti-Soviet spokesmen in the West. This biography is far from being the last word on its protagonist, but those produced in the future will assuredly draw on it.

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NEAR EAST

A. A. DURI. *The Rise of Historical Writing among the Arabs*. Edited and translated by LAWRENCE I. CONRAD. Foreword by FRED M. DONNER. (Modern Classics in Near Eastern Studies.) Princeton: Princeton University Press. 1983. Pp. xxi, 191.

The Rise of Historical Writing among the Arabs is a translation of A. A. Duri's *Bahth fī Nash'at 'Ilm at-Tārīkh 'inda'l'Arab*, published originally in Beirut in 1960. This work by a leading Arab historian represents an attempt to trace the origins and early development of historical writing among the Arabs during roughly the first three centuries of Islamic history. In its original Arabic edition, the second half of Duri's work consisted of a substantial collection of extracts from early Arabic historical sources included for the purpose of illustrating "the style, method and content of the . . . writings" of early Arab historians. The translator, Lawrence I.

Conrad, chose wisely, I believe, to omit this part of the work from the English translation, since its value as illustrative material derives largely from its Arabic form. The notes and the bibliography have been corrected and considerably augmented in the English edition of the work. It should also be noted here that the English translation was reviewed in its entirety by Duri himself prior to its publication. Conrad is to be commended for having done an excellent job of rendering what has to be described as a difficult Arabic text into clear, readable, and accurate English. The translation is prefaced by a useful introduction in which Fred M. Donner reviews the progress achieved in the study of Arabic historiography over roughly the last one hundred years.

When viewed within the context of the modern study of Arabic historiography, Duri's work clearly represents the culmination of a long series of attempts to trace the development and assess the value of the tradition of history writing in early Islam. It carries forward the work of M. J. de Goeje, Julius Wellhausen, Ignaz Goldziher, H. A. R. Gibb, Nabia Abbott, Franz Rosenthal, and others over the past century. Although he is indebted to the scholarship of an earlier period, Duri's work constitutes, in a variety of ways, a significant advance in the study of Arabic historiography. First of all, it provides the most comprehensive overview of early historical writing among the Arabs that has yet appeared. Secondly, despite the fact that Duri takes cognizance of the work of earlier scholars, his analysis and assessment of the tradition of history writing in early Islam are based to a very large degree on his own independent research carried out over a period of several decades. Finally, the work is significant in that it was written by one who is himself a practicing historian, that is, by one concerned above all with the reliability of his sources. The result of this effort is not only a careful reconstruction of the tradition of history writing in early Islam, but also the elaboration, largely *de novo*, of criteria for assessing the authenticity of Arabic historical sources. In general, it can be said, Duri agrees with Abbott in emphasizing the reliability of Arabic historiography.

Since this is not the place for a detailed review of Duri's work, I shall limit my critical comments to a single issue, albeit one that is important for an understanding of early Islamic history and historical writing. Duri, it seems to me, draws much too sharp a distinction between the religious (Medinan) and the tribal (Iraqi) perspectives in early Arabic history writing. Although our early Arabic sources do indeed reflect two such perspectives, these latter are not to be thought of as two distinct and separate schools of thought but rather as two tendencies that were frequently in contact with each other producing a variety of intermediate points of view. Iraqi

historiography, although profoundly influenced by tribal interests and perspectives, also felt the impact of the religious point of view. Indeed, the very existence of the tribes in Kufa and Basra was the result of Islamic factors that must have been evident to early Iraqi historians. Likewise in Medina, the tribal dimension of Muslim social existence was a fact of life that impinged on the religious outlook at critical points. In short, then, the outlook reflected in our early Arabic historical sources is to be seen as the result of the interaction and interpenetration of the tribal and religious perspectives.

Despite the above remarks, this work, taken as a whole, represents an important step forward in the scientific study of early Arabic historiography and is to be recommended as an important resource for those interested in early Islamic history.

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MICHAEL G. MORONY. *Iraq after the Muslim Conquest*. (Princeton Studies on the Near East.) Princeton, N.J.: Princeton University Press. 1984. Pp. ix, 689. \$42.50.

"It can be argued that Islamic civilization was the result of changes that began under the last Sasanians" (p. 521). Thus does Michael G. Morony conclude his study, implicitly disputing those who hold Arabian, Jewish, or Greek influence to have been most critical for the formation of Islam. He shows, instead, that the lands under control of the Sasanian empire (A.D. 226–651) contributed most to the nascent civilization.

Morony shows more, too: that Islam spread throughout the Middle East because it best incorporated Sasanian characteristics. He closes the book with the observation that "what we call Islamic civilization was, in fact, the regional culture of western Asia. . . . [T]he significance of Islam may be . . . that it was the most successful crystallization of the direction of change during Late Antiquity" (p. 526).

These are radical arguments indeed. They come, however, at the end of a large and learned study that meticulously establishes connections between Iraq in late antiquity (A.D. 300–600) and the same region in early Islam (up to the year A.D. 700). In a sweeping review of institutions, the author shows how Muslim practices derived from Sasanian precedents in the realms of military organization, internal security, property holding, public architecture, court procedure, royal offices, government bureaucracy, justice, taxation, and provincial administration. He shows a similar continuity for relationships between religious and political leaders, between the rulers and ruled, and between members of diverse ethnic and religious groups.

Perhaps the most striking case of continuity, for the reader can see it for himself, is coinage: the last Sasanian *zuzes* and the first Muslim *dirhams* are identical except for a tiny Islamic legend on the margin of the *dirhams* (pp. 42–43). Early Muslim rulers propagated images of Sasanian emperors and Magian fire altars!

Morony also establishes that Sasanian administrative institutions “were adopted by Muslims during the seventh century . . . at least a century earlier than is usually assumed” (p. 18). It was not the caliphs but the Muslim governors of Iraq, and especially Ziyad b. Abihi, governor of Basra from 665 and of all the eastern provinces from 669 until 673, who first applied Sasanian theories and practices to Islamic government.

In the four centuries under consideration, the author shows, Iraq was a great center of religious change. He breaks the custom of studying Iraq’s religions in isolation and looks at them together, proving that major developments took place not only within each religion (such as the Nestorian schism and the establishment of Rabbinic Judaism) but also in state-church relations. The practice of government toleration for a multiplicity of faiths came from Iraq, as did the notion of ultimate allegiance directed to one’s religious grouping, so essential to the later Muslim political order.

The author, who seems to have read everything bearing on Iraq in these centuries, argues his case with great persuasiveness. Although *Iraq after the Muslim Conquest* is but a “preliminary statement” (p. 23) in an ongoing project, Morony has already laid down his challenge: will anyone argue that Sasanian civilization was not critical to the formation of Islamic civilization?

DANIEL PIPES
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ANDREW M. WATSON. *Agricultural Innovation in the Early Islamic World: The Diffusion of Crops and Farming Techniques, 700-1100*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1983. Pp. x, 260. \$39.50.

The thesis offered by Andrew M. Watson, an economic historian, is that an agricultural revolution occurred in the area between Iran and Spain in the early centuries of Islamic conquest and expansion. This revolution involved not only the introduction of a large number of new food plants and a fiber plant, but also the development of new principles of cultivation, land tenure, and human relations. Far from having destroyed or degraded the agricultural systems they found, as one stereotype maintains, the Arabs encountered systems already crumbling in the Byzantine, Sassanian, and Spanish territories

and restored them to a high level of prosperity through an integrated and highly sophisticated agriculture, which was considerably in advance of that of contemporary feudal Europe.

Most of the new crops came initially to the Near East from India, with a few from Africa. Watson selects the plants he believes most significant: sorghum, Asiatic rice, hard wheat, sugar cane, Old World cotton, sour orange, lemon, shaddock, banana, plantain, coconut palm, watermelon, spinach, artichoke, colocasia, eggplant, and mango tree. He carefully documents the known chronology, routes, and mechanisms of diffusion of each plant from its homeland into the Islamic world. In his treatment Watson has used a wide variety of sources, including many original texts—travelers’ accounts, Arab botanical treatises, medieval cookbooks, literary references, artistic representations, linguistics, and archaeology—to reconstruct a brilliant if ephemeral world. The arguments are clearly stated and are meticulously documented by references and supplementary footnotes. The sprinkling of anecdotes and interesting tidbits makes for enjoyable reading. The illustrations are charming.

Watson sees the substantial demand for these new products as arising through (1) the frequent migrations of peoples across the Islamic world in Umayyad and Abbasid times, bringing their native tastes with them, (2) the influence of rulers and their courts in promoting ostentatious consumption, and (3) a “real demographic revolution” (p. 129) with rising rural and urban population levels that often forced cultivators to farm more intensively with irrigation and shorter fallow periods and to bring marginal land into use. Although Watson does not make the point, this picture is very consistent with the thesis that population pressure evoked agricultural intensification, a view argued by the Danish economist Ester Boserup in her book *The Conditions of Agricultural Growth* (1965). Indeed, population decline in the later centuries seems to have been a major cause of the breakdown of Islamic agriculture (which, he surmises, had overreached itself and become vulnerable to climatic and political disruptions) and of the loss of some of the crops introduced earlier. The case of Spain after the Christian reconquest illustrates this nicely. Here the flight or death of much of the Muslim population made the old labor-intensive crops less attractive, and they were gradually replaced by the traditional crops of feudal Europe—grains, pulses, and vines (although, of course, cultural biases of newcomers from Christian Europe also probably played a role). Elsewhere in the Islamic world, raids by nomads, large-scale invasions, and epidemics seem to have produced similar demographic and agricultural declines. The diversion of traditional trade routes and discovery of the New World were but the final blows.

The volume will be valuable not only to historians and economists but to botanists, geographers, anthropologists, archaeologists, and all scholars interested in medieval and Islamic societies.

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MARIAN KENT, editor, *The Great Powers and the End of the Ottoman Empire*. Boston: George Allen and Unwin. 1984. Pp. x, 237. \$29.95.

Seldom is a collaborative work as carefully organized as this one. Six essays of approximately equal length, each on a single great power's policies and actions with regard to the Ottoman empire, follow a nearly uniform pattern. They discuss the power's general interests; its specific political, strategic, economic, and cultural interests in Turkey; its official, especially diplomatic, personnel concerned with the relationship; the course of its interactions with the empire between about 1900 and 1918 (four go on to 1923 or so); and its responsibility for Ottoman weakening and collapse. F. Roy Bridge writes on Austria-Hungary, R. J. B. Bosworth on Italy, Alan Bodger on Russia, Ulrich Trumpener on Germany, L. Bruce Fulton on France, and Marian Kent on Britain. The seventh member of the concert of Europe, the Ottoman empire itself, which was both actor in its own right and object of the diplomacy of others, is described in somewhat broader terms by Feroz Ahmad.

Although five of the authors—Ahmad, Bosworth, Bridge, Kent, Trumpener—have earlier published on related subjects or on the same period, each chapter provides some new information and interpretations as well as an up-to-date summary. Ahmad's task is the most demanding. He surveys Ottoman weaknesses, starting with 1774, and Ottoman methods of dealing with the powers, especially under Abdul Hamid II and the Young Turks; he cannot go as deeply into diplomatic questions as the others. Nor, although he uses published Turkish materials, has he dug into the Ottoman archives. The other authors, excepting, of course, Bodger on Russia, where access is virtually impossible, exploit the archives of their respective powers. Bosworth on Italy uses archives less than the other four. His subject is also the least in importance; in compensation, his essay is the most delightfully written. Bodger on Russia and Fulton on France probably provide the most that is new, even though Jacques Thobie has recently covered a good deal of the French ground. Each is strong on economic interests: Fulton on French financial involvement in the Ottoman empire, a story sometimes hard to follow, and Bodger on Russia's need to keep the Straits open to her commerce.

Bridge makes clear that the Habsburg and Ottoman empires, despite common problems, were never close; the Habsburgs had always to consider the Balkan states as well as Turkey. Trumpener shows that Germany, trying to advance her investments and trade, never reaped the rewards she hoped for from her support of the Ottomans, even in wartime. Kent analyzes the British policy of maintaining the status quo in Ottoman lands, countering Germany and containing Russia, followed by an abrupt turn toward partition in 1914 and after.

Clearly all the powers helped weaken the Ottoman empire, although in the war and postwar years the British did most. Responsibilities are shaded; Fulton and Bodger have some wise concluding observations on this. But a "composite picture" (p. 3) is never drawn, although the four-page introduction offers a brief summary statement. The impact of Europe as a whole on the empire was greater than the sum of the six powers' actions. Europe's post-1911 jag of political alcoholism, as Sir Edward Grey retrospectively called it, particularly devastating to the Ottoman empire, does not come through clearly, although Giovanni Giliotti's presentiment thereof is remarked (p. 60). This volume is not the magisterial reexamination of the Eastern Question between 1900 and 1923 that the title might lead one to expect; it lavishly supplements but does not quite replace Harry Howard's *Partition of Turkey* (1931). Yet every essay in this volume is of high quality. Further, each is thoroughly documented, the British and Russian ones overwhelmingly so. Scholars will rejoice at the concise summaries, the considered interpretations, and the copious references.

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JUSTIN MCCARTHY. *Muslims and Minorities: The Population of Ottoman Anatolia and the End of the Empire*. New York: New York University Press; distributed by Columbia University Press, New York. 1983. Pp. xii, 248. \$35.00.

This work is a welcome addition to a rapidly growing branch of Ottoman historical studies—historical demography. Justin McCarthy comes well prepared to this having already done considerable work in the area. The special place this work will occupy in the literature is suggested by one of his conclusions (p. 139). Although some have compared the Turkish republic to a phoenix rising from the ashes of the Ottoman empire, McCarthy points out that the analogy is inaccurate since the phoenix, before and after, was the same bird, but Anatolia in 1922 was, demographically, quite different from Anatolia in 1911. This work presents considerable statistical evidence of the great changes that took place.

Mere statistical expertise would not suffice to deal with this subject, since much of the correction of data involves very nonstandard deviations due to specific historical circumstances. The author presents these before turning to the data so that the reader will understand the author's corrections. Certain cautions apply to the population as a whole, others to special groups or areas. In Anatolia, he notes, women and children were generally undercounted. When dealing with time series data for each province, one must ascertain for every year listed which district towns have been added or excluded since the previous census, because changes occurred frequently. In addition to the natural differences between the *vilayets* (provinces), for example, various social aspects, such as health or occupational structure, influenced demography. Because the Ottomans grouped their population into *millets* (religious groups), data for many groups (for example, non-Turkish Muslims, Greek Catholics, and so forth) are hard to isolate.

In his preface the author asserts that he wants "to put the lie to the often said statement that there are no population statistics of the Ottoman Empire." This is a claim the reviewer has not encountered, the more usual one being that available statistics are of little reliability. To prove his point, McCarthy has made extensive use of the *salnames*, the statistical yearbooks of the Ottoman provinces, an innovation of the late nineteenth century. As the reviewer knows well from personal experience, tracking these down is not easy, and McCarthy is to be congratulated on having located so many of them.

The most disputed points in the history of the region in this period concern the size and distribution of certain minority populations, especially the Armenians and the Greeks. The author is generally quite skeptical of figures supplied by West Europeans or Russians, contending that they did not use the Ottoman statistics at all. When analyzing the figures presented by the Armenian Patriarchate, the author makes several interesting points, some of which raise other questions. He notes that the figures are round, which is never true of exact figures, and then explains this by saying that these figures came in response to the following questions: "Approximately how many Armenians would you say were in the eastern vilayets?" (p. 55). [The question is in quotation marks in his text, but it is not clear what the source for this is.] He notes that no specific rules from the patriarchate for collection of birth, death, and marriage records have been discovered; no Armenian parish records have come to light; the patriarchate figures were not compiled on the basis of baptismal records; and, finally, "It is doubtful" that the Ottomans would have allowed the Armenians to engage in mass data collection. Much of this is argumentation *ex silentio*, and here McCarthy is on

thin ice; soon after (p. 55), he acknowledges, "That some form of baptismal records existed . . . is undoubtedly true." Someone else might ask how likely it is that, in the flush of victory, the Ottomans would have attempted to save Armenian records they found?

The other ethnic group of central importance is the Greeks, who generated statistics about which McCarthy has some interesting observations. Many Greeks would not appear in the Orthodox *millet* figures, since several million were Catholic and more held foreign citizenship. He is very skeptical of Greek figures but asserts that accurate official figures on the Greeks were "in the Ottoman's self-interest" (p. 90). He notes, however, that at Versailles the Greek and Turkish figures were never compared (p. 90). For collection of taxes on non-Muslims, a larger number of Greeks would be in the Ottomans' interest but, at international conferences, perhaps not.

The only methodological weakness of the work is not essentially a weakness of the author as much as a characteristic of the training of Ottomanists since the field took off in the early 1960s: specialists are often sent out to work in the field without training in the languages or history of any of the subject peoples, and the young specialist is conditioned to consider this a normal and fully acceptable state of affairs. McCarthy has, of course, consulted sources of the non-Ottomans in Western languages, but one might wish that the exhaustive approach he tried with Ottoman material had been tried with other sources as well. It may be that in the future such studies in historical demography should be carried out collectively.

More careful reading by the press could have prevented the appearance of statements such as: "a grant . . . greatly helped to defer publication expenses" (preface); or, "Yet the populations of the United Kingdom and Germany actually rose between 1911 and 1912" (p. 121). McCarthy has thought much about the appalling losses all groups suffered. The material he has presented should make the reader do likewise. His text amply supports one of his conclusions: "To mention the sufferings of one group and avoid those of another gives a false picture of what was a human, not simply an ethnic, disaster" (p. 137).

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LEILA TARAZI FAWAZ. *Merchants and Migrants in Nineteenth-Century Beirut*. (Harvard Middle Eastern Studies, number 18.) Cambridge: Harvard University Press. 1983. Pp. x, 182. \$20.00.

Beirut has shown us the complex features of civil war and external intervention in the Middle East. These have been tragic legacies for the last of the cosmopolitan centers of the Eastern Mediterranean, whose nineteenth-century origins owed much to the capitulations, the special framework within which Western influence made itself felt in the Ottoman empire. The system of extraterritorial privileges, second only to those of an imperialist occupation, encouraged Europeans (and Americans) to transplant themselves into the Islamic world in hothouse conditions that could not withstand this century's strong winds of change. Before Beirut there was Alexandria, before Alexandria, Smyrna, and before that Istanbul. Leila Tarazi Fawaz notes perceptively that Beirut differed in a very important respect. Where, for example, Alexandria came under the economic domination of Westerners, Beirut internalized their influence to a greater degree and produced relatively more of its own adept practitioners of commercial enterprise. With the consequence, one might add, that where elsewhere some of the tensions associated with such enterprise were alleviated, justifiably or not, by the expulsion of the foreigner, Beirut has been precluded from such ready relief.

Relying on a wide variety of Western and Middle Eastern archival sources, private papers, local histories, chronicles, interviews, and the contemporary publications of a large number of nineteenth-century Western observers, Fawaz has painstakingly pieced together statistical estimates for the growth of Beirut from a small town of some six thousand early in the century to the region's chief port city on the eve of World War I with a population of 150,000 to 200,000. The author's thesis is that, whereas Western seaborne economic activity generally favored the Eastern Mediterranean coast over its hinterland, conditions special to the region account for Beirut's growth over other eligible coastal towns. In the 1830s Muhammad Ali's "modernizing" Egyptian administration found it convenient to make Beirut, nestled in the shadow of Mount Lebanon under the authority of an allied amir, the administrative center of the territories conquered in the first Syrian war. In turn, high-level European consular representatives followed suit, becoming permanent fixtures and conduits for the capitulatory advantages enjoyed by European and local entrepreneurs after the area returned to Ottoman hegemony. Such developments made Beirut a magnet for internal Christian migration from the Syrian hinterland and from Mount Lebanon where established socioeconomic patterns governing the subordination of Christians to Muslims or Druzes were breaking down under the pressures of complex change.

Social tension, civil strife, and fear of massacre brought different waves of migrants to Beirut as

refugees or for relocation, thus building up the city's ratio of Christians to Muslims. Fawaz calculated that this went from an estimated 45 to 45 percent split in 1838 to an estimated 54 to 34 percent division in 1912. Religious affinities with Europeans, reinforced by European-sponsored educational opportunities, gave local Christian merchants the initiative with which to equal or surpass Western entrepreneurs in the economic development of Beirut, many under the ubiquitous umbrella of capitulatory privileges. Beirut's development, however, also attracted a steady, though smaller, stream of Muslim settlers some of whose merchant elite assisted the Christians by providing them with contacts in the Muslim hinterland to which European entrepreneurs could not aspire. European, Christian, and Muslim links at commercial and other levels helped the city prosper to a degree, but antagonisms were transplanted and dislocations adverse to Muslim interests persisted. This produced uneasy sectarian relations with subtle rivalries at the top of the socioeconomic scale and serious flare-ups, resulting in fatalities, lower down. Nevertheless, the author sees the common denominators in Beirut's sectarian society as the predominant feature of the nineteenth century, and she hints at their ultimate legacy of hope in the current crisis.

Fawaz's timely monograph provides an informative and readable framework for the study of the modern history of Beirut. The book focuses on the middle years of the century when the city achieved "critical mass" and on analytic sketches of leading foreign and indigenous entrepreneurs, and its brevity (124 pages of text) permits only allusions to related developments. It is a tribute to the author's insight that this reviewer's regrets about the work are confined to what lies beyond these limits. Migrants to the city and its European and local entrepreneurs were at the center of political, intellectual, and international developments in the nineteenth century that now merit further "fleshing out." Ultimately these individuals made Beirut, in its French-inspired Lebanese incarnation, something that can be described as the last of the Mediterranean city-states and by the same token the central battlefield of its civil war.

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AXEL HAVEMANN. *Rurale Bewegungen im Libanon-gebirge des 19. Jahrhunderts: Ein Beitrag zur Problematik sozialer Veränderungen.* (Islamkundliche Untersuchungen, number 79.) Berlin: Klaus Schwarz. 1983. Pp. xxii, 433.

Peasant unrest is not an unusual phenomenon in Middle Eastern history. But nowhere did it evolve

into such a sustained movement and cause so much change in the political structure as in the Mt. Lebanon region during the nineteenth century. This period is also one of the few in modern Middle Eastern history for which several well-informed studies already exist.

The subjects of Axel Havemann's study are the three rural protest movements of 1821, 1840, and 1858–61. The aim is to reconstruct the historical record of events through an exhaustive use of all accessible materials, to evaluate and analyze the three movements, and, at the same time, to revise some of the conclusions of earlier studies.

In a large background chapter Havemann succeeds well in describing the demographic, social, and economic particularities of society in the Mt. Lebanon region. He argues convincingly that, in contrast to all other areas of the Middle East, a feudal society indeed existed in the Mt. Lebanon region. He shows that society was organized in a secular political framework of feudal lords regardless of sectarian affiliation.

His analysis of the very complex issue of the demographic expansion of the Maronites raises some questions. It was a dynamic process reaching back into the seventeenth century and constituted one of the causes for later unrest. "High population density" (pp. 27, 220) is a very relative concept and certainly does not explain per se why Maronites migrated south and why Maronite population increased relative to Druze population. "Political pressure" to migrate (p. 16) is a more convincing argument. But one major cause is not mentioned at all: the role of Maronite monasteries in cultivating new lands in the south, which attracted Maronite peasants from the north and gave them greater prosperity, longer life expectancy, and probably higher fertility.

In the main part of the study Havemann is particularly concerned with the rise of a new political consciousness and a concomitant form of organization, best expressed by the word *cammiya*, a term that in Arabic means general public or commonwealth as well as common people. The term provided a new source of legitimacy as well as describing the social origins of the participants. Perhaps the most remarkable aspect of these rural movements is that they were led and sustained by people of low social class. Havemann's argument about the precise social origins of the leadership can only be settled satisfactorily when more biographical information is found (perhaps church records would help here).

What becomes clear from Havemann's description is that the villages around Mt. Lebanon were much more socially stratified and differentiated than those in other parts of the Middle East. Sericulture stimulated spinning and weaving in the off-season. Peasants and artisans, clergy and feudal lords, small

landowners and landless peasants—all lived in the same village. Probably this accounts at least partially for the ability of the villagers to organize and sustain political opposition movements and for their ability to formulate new political programs.

Although Havemann perceives an evolution of political thought and participation in the three episodes of 1821, 1840, and 1858–61, he is careful to point out that the role of the Maronite Church was of increasing importance and gave the movements more of a sectarian character and that the *cammiya* character of the movement did not lead in the long run to a secular national identity or socialist forms of organization. The movements did not constitute revolutions but "archaic social movements" (p. 274).

On the whole, the study furnishes a thoroughly researched and well-balanced evaluation of these rural movements and their meaning for the general history of the region.

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PHILIP S. KHOURY. *Urban Notables and Arab Nationalism: The Politics of Damascus, 1860–1920*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1983. Pp. xi, 153. \$34.50.

Great movements seem to necessitate finding notable and striking origins for them. Arab nationalism, whether regional or pan-Arab, has been the dominant political movement in much of the Middle East in the twentieth century. The origins of Arab nationalism have been a subject of study for nearly seventy years. During that time two main approaches have emerged: the first holds that Arab nationalism came from a dramatic Arab cultural renaissance well before World War I and that most Arabs became nationalists at a relatively early time; the second, a revisionist school of thought, argues that the rise of Turkish nationalism after 1908 effectively began Arab nationalism and most Arabs were converted to it only well after World War I and the epochal end of the Ottoman empire. Philip S. Khoury's short monograph provides cogent, concise, and well-argued evidence to support the second point of view.

Arab nationalism was strongest in Beirut and Damascus in the last days of the Ottoman empire. Khoury systematically looks at the landowning and bureaucratic elite families of Damascus in the last fifty years before World War I and finds reasons why some members of this group became Arab nationalists. As the men of religion comparatively lost social status and the local military leaders joined the commercial elite, one interrelated group of landowning bureaucrats dominated local Damascus

politics. The composition of this group, its marriage patterns, commercial activities, patronage links, and general behavior are carefully traced in detail drawn from local chronicles and interviews with descendants. Following the 1908 revolution in Constantinople, some of the notables, particularly those who were out of office, and others who were young, professionals, and from the poorer branches of rich families, split from their fellow notables by favoring Arabism within the framework of the Ottoman empire. This group and young nationalists from Iraq and Palestine later dominated the post-World War I government of the Amir Faysal in Damascus, although most of the old guard among the notables remained cool to him.

The chief value of Khoury's book lies in its confirmation of earlier analyses of the origins of Arab nationalism by C. Ernest Dawn and Zeine N. Zeine. At the same time, Khoury tests and helps validate the ideas of Albert Hourani, who maintained that in order to understand thoroughly local and regional Middle Eastern political history, the economic and social role of urban Muslim notables must first be investigated.

Little new information is provided in two of the four chapters that deal with the pre-1860 situation and the Arab government of 1918-20. A greater weakness is the absence of information on Damascus drawn from the Ottoman imperial archives. Khoury does effectively marshal valuable information, however, from printed Ottoman provincial yearbooks for Syria. The concept of economic class is at first employed, but it is severely limited in its application to the Damascus elite, and then later Khoury firmly, and appropriately, rejects it in favor of an analysis of political beliefs and behavior based on considerations of family, patronage, office holding, age, and education.

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ROBERT L. TIGNOR. *State, Private Enterprise, and Economic Change in Egypt, 1918-1952*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1984. Pp. xvi, 317. \$40.00.

This is a well-written work that traces the economic development of Egypt during the first half of the twentieth century, recounting the rise of an industrial bourgeoisie that sought to diversify industry and diminish Egypt's dependence on one crop, cotton. Although none of the material is unknown to the specialist, the theoretical approach and the cogent analyses make this book essential reading for the student of Egyptian history.

Nevertheless, there seems to me to be a basic contradiction in Robert L. Tignor's thesis that a local

bourgeoisie of foreigners resident in Egypt worked hard to effect real structural changes in the Egyptian economy (p. 251) and were not the parasitical group dependency theoreticians describe. He maintains that far from "supporting the economic status quo they [alien residents] worked aggressively for its alteration," (p. 7), as did the native bourgeoisie. Yet, although the native bourgeoisie was activated by nationalism, hoping to make their country economically independent, the author clearly states that the foreign residents "always endeavoured to maintain close ties with overseas capital . . . were fearful of radical nationalism, with its threats to their privileged status . . . stressed their attachment to European culture and made little effort to hide their contempt for the Egyptian, Arab and Muslim heritage of the country" (p. 6). Furthermore, he underlines the alien residents' "profound prejudice against employing Egyptians or promoting them into responsible high-level positions" (p. 208). He adds that entrepreneurs in Egypt "founded few consumer durable factories and even fewer firms concerned with the production of intermediate goods and capital goods" (p. 205). This leads one to ask in what way they were not parasitical. Or, in the author's own words, "did the presence of foreign entrepreneurs . . . divert development into areas which made the country reliant on Europe and less able to determine its own economic direction?" (p. 23).

Tignor's answer to that question is that the bourgeoisie, native and foreign, failed to create conditions conducive to economic diversification, and the state was slow to implement programs recommended by the native bourgeoisie. Yet he attributes such failure to "the populist and anti-foreign nature of Egyptian nationalism which rendered difficult a strategy based on using foreign assistance to overcome domestic lacks in capital and skills" (p. 247). Throughout the rest of the book Tignor describes how Egypt was despoiled by foreign capital (pp. 182-83), how the alien residents appealed to their countries to protect their privileged positions so that little competition arose, and how alien residents were unconcerned for Egypt itself except as a comfortable and convenient money-maker. Can one believe, then, that the alien residents served the country when they refused to use or train Egyptian talent? Far from disproving dependency theories, this work goes a long way toward justifying them.

A more careful editing of the book would have avoided the numerous typographical errors and the errors in transliterating Arabic names.

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MOHAMMAD A. TARBUSH. *The Role of the Military in Politics: A Case Study of Iraq to 1941*. Foreword by A. H. HOURANI. Boston: Kegan Paul International. 1982. Pp. xvii, 285. \$28.95.

This book is a revised version of a Ph.D. thesis submitted to the University of Oxford in 1978. It is the most recent in a series of works in the last three decades on the role of the military in politics. A foreword by Albert H. Hourani is followed by five chapters of introductory details, covering two-thirds of the text, on the conditions of Iraq before the first military coup of October 1936. The author describes Iraq's demographic structure, its political system, the organization and composition of its army, and the military's role in the operations against the Assyrians and the tribal uprisings of 1935-36. The critical details are accompanied by useful tables of prime ministers, members of parliament, and senior officers with their background, religion, and place of birth. The remaining three chapters of the book discuss the military interventions in the politics of Iraq from the first Bakr Sidqi coup of 1936 to the four officers' coup of April 1, 1941 that imposed Rashid Ali al-Gailani as prime minister. The book ends with thirty-four pages of appendixes dealing mostly with the Anglo-Iraqi treaty of June 30, 1930, and the correspondence relating to it between the two sides.

Mohammad A. Tarbush has based his study primarily on his findings in the British archives and to a lesser extent on Iraqi archives, Arab memoirs, and secondary Arab and English sources as well as some interviews with survivors of the events. Actually, the most important feature of this work and perhaps its main contribution is its extensive quotations from Foreign Office and other British files in which high-ranking British officials comment on Iraq's political system and evaluate the political and social behavior of Iraq's leaders. Tarbush, however, might have gone too far in depending on some of these comments for his analysis of events, and the same applies for the memoirs of Colonel Salah al-Din al-Sabbagh.

The author could have included in the text much of the information he chose to include in the footnotes, such as the material of notes 68, 72, and 103 in chapter 6 that relates to the impact of the Palestine problem, the Jews in Iraq, and the assassination of Bakr Sidqi. He could also have given more explanation of the way Jafar al-Askari's life ended. The differences in the way the coups were carried out—by violent action or simply by military pressure and threats—could have been described. Finally, the author can be reminded that the name of the Palestinian nationalist on page 138 is Mu'in al-Mādi (not al-Mohdi) and that the CENTO mentioned on

page 140 was known as the Baghdad pact until 1958.

In spite of the preceding remarks, this well-written work with its rich details and documentation remains a useful contribution to the literature on the early history of independent Iraq and on the military in politics.

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AFRICA

JOHN ILIFFE. *The Emergence of African Capitalism*. Minneapolis: University of Minnesota Press. 1983. Pp. ix, 113. Cloth \$29.50, paper \$10.95.

In evaluating the origins of African capitalism, John Iliffe has modified the definition of capitalism familiar to readers of Adam Smith. Definitions of terms such as capitalism vary considerably from decade to decade and country to country. Iliffe develops a definition that seems best suited to the African experience (see my *British Business and Ghanaian Independence*, pp. 3, 116-19). He does this by examining several locations in Western, Eastern, and Southern Africa to illustrate the variety of settings and types of capitalist development unique to Africa.

As capitalism penetrated Africa from overseas during the nineteenth century, wage labor became its characteristic component. Iliffe and others who trace the history of African economic forms focus on this pattern of hired labor as the defining feature of African capitalism rather than use the more complex European system as a model.

Factors shaping African capitalism, according to Iliffe, are: firstly, the history of the older African societies; secondly, social diversity, which led to economic patterns in Western Africa distinctly different from those of Eastern and Southern Africa; thirdly, the existence of capitalism in West Africa prior to its appearance in the eastern part of the continent, which led to more rapid economic development in the West; and finally, underpopulation, ample land, autonomy, and African pragmatism, all of which also helped to mold African capitalism.

The themes investigated by Iliffe include the effects of hired labor on peasants, the impact of both Islamic and Christian missionaries, and the evolution of capitalism after World War II, as the African nation-states developed. He also examines the interaction of politicians and capitalists. Countries Iliffe examines include Nigeria, Ghana, Kenya, Senegal, Tanzania, Ivory Coast, and Zambia.

In the early colonial period, according to Iliffe, foreign capitalism swamped African commerce and agriculture. Only after 1945, following changes in the nature of international capitalism, did African industrial growth begin. The new states—including Nigeria, Kenya, and a dozen others—played a central role in postwar industry. This growth, however, led to organizational problems for African entrepreneurs.

In these essays, based on his Anstey Memorial Lectures, delivered at the University of Kent in May 1982, Iliffe provides us with a most significant set of measures and characteristics of African capitalism.

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NANCY ELIZABETH GALLAGHER. *Medicine and Power in Tunisia, 1780–1900*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1983. Pp. xii, 145. \$39.50.

This study uses great epidemics, a familiar device from medical history, as a probe to explore several aspects of Tunisian history. Nancy Elizabeth Gallagher describes plague epidemics in 1784–85 and 1818–20, cholera in the mid-nineteenth century, and typhus in 1868. She organizes her study around three questions: the direct demographic and economic impacts of the epidemics, evidence of social and intellectual conflicts generated by health crises, and changes in medical theory and practice.

The author avoids the temptation to exaggerate epidemiological factors, and her conclusions appear sound. Epidemics, especially plague, caused much loss of life, but they neither depopulated Tunisia nor made more than a minor contribution to the country's economic problems. Medical, political, and religious leaders could do little to protect the population, but their failures did not produce substantial public questioning of the established order. A small elite group, however, gradually became convinced of Western medical superiority, and the support of this modernizing elite caused the growing prestige and power of European doctors and medical ideas. This was not, as Gallagher so correctly points out, because of any inherent superiority of contemporary Western over Islamic medicine. Indeed, for much of the period, practitioners of the two traditions shared a common Galenic heritage and were willing to learn from each other. Native and European doctors argued among themselves about the miasmatic or contagious nature of epidemics, but neither group could prevent or cure them. Even in the 1870s the European domination of official medicine owed more to political factors and the elite's perception of overall European scientific and tech-

nological prowess than to any obvious superiority in therapy.

Gallagher's focus on epidemic crises is valid but perhaps a bit narrow. We learn almost nothing about unspectacular endemic diseases, which were probably more important to the health of Tunisians in the long run. Perhaps European doctors were able to impress the elite with successes against chronic illnesses or trauma; they certainly had nothing but quarantines to offer against cholera. Discussion of the crucial pre-Protectorate decade of the 1870s is quite brief, and the early years of French colonial medicine receive only cursory treatment.

These criticisms detract little from the merits of this well-researched monograph. Short, clearly written, and quite accessible to nonspecialists in medical or Islamic history, it will be useful to historians interested in medicine, North Africa, or European cultural and political expansion.

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Charlotte

J. D. Y. PEEL. *Ijeshas and Nigerians: The Incorporation of a Yoruba Kingdom, 1890s–1970s*. (African Studies Series, number 39.) New York: Cambridge University Press. 1983. Pp. xiii, 346. \$57.50.

Recent African anthropology and historiography appear to have come full circle: from the view that Africans have no history because they have no written records to the suggestion, which is sometimes made, that they have no history because their lives are being directed from elsewhere. From the perspective of "dependency theory," or "world systems analysis"—which, in truth, are variations on the old Leninist theme of imperialism—Africans, and indeed much of the Third World, exist only to the extent that they are the "periphery" of a capitalist "center." And what happens at the periphery is, of course, determined by the center. J. D. Y. Peel, professor of sociology at the University of Liverpool, rejects this notion as well as the opinion that local reality is but a variation of a universal pattern; communities, like individuals, have their predispositions and their values, and the past cannot simply be relegated to a "myth" or a useless "context."

One of the major strengths of this community study and social history of Ilesha, a Yoruba town of some 180,000, is that the work not only attends to the wider forces impinging on the community but also views Ilesha as responding in its own manner from its own perspective. Thus, Ilesha is not only "incorporated" by the Nigerian state and the world economy, but also interprets, maneuvers, and seeks out allies and resources that enable it to endure according to its own values.

For example, when in the 1950s and 1960s much of Yorubaland supported the Action Group party (AG) of Chief Obafemi Awolowo, Ilesha decided to oppose the trend and to support the National Council of Nigeria and the Cameroons (NCNC). One explanation for this deviation from a wider trend was offered by Richard L. Sklar in his notable study of Ibadan. Sklar suggested that the lower classes, who were by and large Muslim, opposed the AG, seeing in it the power of the Christian middle and upper classes. Support for the NCNC was, therefore, a form of class protest. Taking a very different tack, Peel demonstrates that, in the first instance, Ilesha turned to the NCNC because its Oba had been at odds with the Oni of Ife, who had himself supported the AG. Thus, a division that had its roots in tradition, indeed in precolonial history, appeared once more as a contemporary form of conflict.

Peel's goal is not to substitute the reductionism of tradition and communalism for that of modern capitalism and class conflict. He tries to demonstrate, in this and other examples, that tradition and modernity, communalism and class allegiances are not always or primarily in conflict. He suggests that they be viewed as complementary, as coordinates, useful in tracking the actions of people in the process of change.

Thus, the same Ijeshans who had supported the NCNC against Awolowo's AG, refused to support a political party—a successor to the NCNC in the western region—that was viewed as being in league with the enemies of the Yorubas. Indeed, in the changed circumstances, Ijeshans stood among the stalwarts of Awolowo and the AG. The point is that Ijeshans were willing to use one set of value criteria—traditional loyalty—for one decision and a different set—allegiance to Yoruba ethnicity—for another. Both sets of criteria are needed to understand their behavior in changing circumstances.

Beyond engaging some important intellectual and historiographical issues in the literature on Africa, this study is carefully crafted. Based on some two years of field work, including extensive interviews and ten years of documentary research, this is a book that makes an important contribution to our understanding of modern Africa and of its many transformations.

ROBERT MELSON
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CLAIRE C. ROBERTSON. *Sharing the Same Bowl: A Socio-economic History of Women and Class in Accra, Ghana*. Bloomington: Indiana University Press. 1984. Pp. x, 299. \$22.50.

Claire C. Robertson is a pioneer both in African women's history and in her ability to use that history

to contribute to theoretical understanding of class formation. This is one of the most important books on African women to have appeared in the last fifteen years.

The book is a socioeconomic history of Ga women in Accra, Ghana, based on surveys conducted in 1971–72 and 1978. The Ga are the indigenous inhabitants of Accra, and the women Robertson surveyed were long-time urban residents. They were also all traders. The book documents their declining status as a result of Ghana's incorporation into the world economy, the extreme inflation of the 1970s, and the growing economic separation of women and men. Robertson's description is supplemented by fascinating life histories of four of her subjects, which provide the analytic chapters with a sense of dramatic reality. There are also plenty of photographs, diagrams, and maps. The book, in short, is a pleasure to read.

Robertson's chief theoretical point is that women's class position must be analyzed separately from that of men. The Ga women of Accra had high divorce rates and serial marriages. They did not, despite an ideology of submissiveness to husbands, keep common property with their spouses, nor did they normally live with them. In fact, Robertson says, the growth in neolocal residence patterns in which younger Ga women seek to live with, and be financially dependent on, wealthier men is an indication of women's declining economic status.

Originally, many of the women were fishsellers, marketing produce that husbands or male kin caught. Many engaged in long-distance inland trade in fish, cloth, and decorative beads. In the postindependence period, however, technology (controlled by men) has overtaken these traders; cold storage has especially undercut the need for smoked fish in Ghana's hinterland. Parastatal retail monopolies have pushed women out of the cloth trade, and in recent years governments have used market women as scapegoats for shortages and inflation. As a result, more and more women traders have been left with little to sell except labor-intensive, low-profit prepared foods.

These women are also increasingly obliged to rely on each other rather than on their menfolk for help. Men rarely fulfill their "traditional" financial obligations to wives and children, yet fertility rates are high; very poor women rely on large numbers of daughters for help in housework, trade, childrearing, and security when they are old. Multigenerational, matrilineal families are common. Yet the women continue to finance the education of sons who do not repay their investments.

Through her study of Ga women traders, Robertson has made a significant contribution to the theory of social class and its pertinence to women. One wonders how far her conclusions can be general-

ized, either inside or outside Africa. Even if no such generalization can be made, however, her central point concerning women's and men's differential access to property, education, and employment is extremely important and merits much further debate.

RHODA E. HOWARD
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ALAN R. BOOTH. *Swaziland: Tradition and Change in a Southern African Kingdom*. (Profiles: Nations of Contemporary Africa.) Boulder, Colo.: Westview or Gower, England. 1983. Pp. xi, 156. \$18.00.

ALLEN ISAACMAN and BARBARA ISAACMAN. *Mozambique: From Colonialism to Revolution, 1900-1982*. (Profiles: Nations of Contemporary Africa.) Boulder, Colo.: Westview or Gower, England. 1983. Pp. xii, 235. Cloth \$25.00, paper \$11.95.

The Nkomati Accords, signed in 1984 by Mozambique and South Africa, symbolized a new phase in the decolonization movement in southern Africa. Within days after the agreement was signed, the press reported that Mozambique's police had raided the homes of members of the African National Congress of South Africa (ANC) and Swazi police had engaged in shoot-outs with ANC freedom fighters. These two books provide some of the historical background necessary to understand these and subsequent events in the new phase in the liberation struggle unfolding at the southern end of the vast African continent.

Alan R. Booth analyzes the way King Sobhuza manipulated traditional ethnic symbols to gain the support of the Swazi peasants and middle class. This enabled him, first in conflict, then in collaboration with the European settlers—who in the nineteenth century acquired over half of Swaziland's territory—to reject the British Westminster model. Thus, after independence, Sobhuza continued to rule as one of the world's last monarchs. As king he taxed the African population in order to repurchase a major share of the lands his predecessors had conceded to Europeans. Simultaneously, he accumulated increased wealth and power for himself and his followers by granting new concessions to transnational corporate affiliates to reap Swazi agricultural and mineral wealth. By the 1980s, when Sobhuza died, drought and international recession were spreading throughout the region, and the Swazi population confronted mounting unemployment and increased poverty.

Unfortunately, perhaps owing to his book's brevity, Booth does not fully develop several aspects of his analysis. These include the way Sobhuza's manipulation of traditional symbols obscured a grow-

ing stratification in both urban and rural areas; the role of foreign capital in accelerating a distorted process of proletarianization of the peasantry; the role of the emerging working class within the opposition, which repeatedly protested Sobhuza's accumulation of wealth and his increasingly autocratic rule; and the way the migratory labor system distorted family life (traditional institutions imposed added burdens on women as de facto family heads). He could have provided at least some evidence to support his repetition of the long-standing myth that privatization of Swazi traditional lands would somehow ensure increased productivity. In his discussion of the implications of the formation of the Southern African Development Coordination Conference (SADCC) for Swaziland and the region, he might have probed more deeply into the way Sobhuza's traditionalism, combined with growing links with South Africa and transnational corporations based there, tended to foster South Africa's pretensions toward creating a viable constellation of states in the region.

Nevertheless, Booth's slim volume does contribute valuable insights into some of the underlying contradictions that led Swaziland's rulers, by the 1980s, to sign a secret agreement, similar to the Nkomati Accords, with South Africa to prevent ANC members from moving through Swaziland.

The Isaacmans' historical analysis of the penetration of Mozambique by colonial capitalism provides an intriguing contrast to the Swaziland case. Over the centuries Portuguese settlers destroyed much of the traditional structure and culture developed by the peoples inhabiting what is now Mozambique. They imposed forced export crop cultivation and created conditions that coerced tens of thousands of Mozambicans to migrate as low-paid labor to South Africa and what was then Southern Rhodesia. Prolonged popular resistance culminated in independence only following a decade of guerilla warfare.

The Isaacmans show that Frelimo, the party that governs Mozambique today, unlike Swaziland's traditional rulers, forged national unity in the struggle for liberation. Its leaders conducted a conscious educational campaign to overcome petit bourgeois elements in general and ethnic divisions and sexist practices in particular.

Mozambique's postindependence development strategy, as outlined by the Isaacmans, has differed qualitatively from that of Swaziland. It has sought to implement a transition to socialism along lines suggested by Marxist-Leninist theory. The Isaacmans discuss the many obstacles that have hindered successful transition. Writing at least two years before the Nkomati Accords, they mention some of the contradictions that may have coerced the Mozambique government into that agreement. They mention, for example, that the Portuguese denial of

education to the African population hindered the development of dedicated cadres to control and direct the modern state. This led to continued reliance on the inherited civil service. They also show that an overemphasis on capital-intensive state farms hindered development of the peasant agricultural sector.

One wishes, however, that the Isaacmans, too, had more fully developed their analysis. Why, for example, did the Mozambique leadership adopt a development strategy that, heavily reliant on borrowed foreign capital, left the overall economy far too dependent on world markets, imported machinery and equipment, and even imported foodstuffs? Why did Frelimo not use the methods it had evolved during the liberation struggle to involve workers and peasants in building small-scale industrial and agricultural enterprises that they could control and that would provide them with employment and raise their living standards in the context of more balanced, self-reliant development? Why did SADCC not, as hoped, help Mozambique to reduce its dependence on—and vulnerability to—South African domination? Nevertheless, the Isaacmans' study does provide some of the background necessary to understand the kinds of factors that led to the seeming policy switch in signing the Nkomati Accords.

In sum, although one might wish the authors had more fully explained a number of issues, both these brief books offer useful introductory historical surveys illuminating the two neighboring southern African states' qualitatively different development paths, which in the early 1980s contributed significantly to the emergence of a new phase in the struggle for liberation in the region.

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ASIA AND THE EAST

JOANNA F. HANDLIN. *Action in Late Ming Thought: The Reorientation of Lü K'un and Other Scholar-Officials*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xiii, 256. \$28.50.

Although not as colorful as some of the Taizhou scholars of the Wang Yangming school in the late Ming times, Lü Kun (1536–1618) has been elevated to a significant position in Chinese intellectual history by Joanna F. Handlin's *Action in Late Ming Thought*. Handlin's main thesis is that a "reorientation" took place in the thought of Lü and others that focused on "personal experiences, self-interest, and local concerns" (p. 218). The first half of the book is devoted to an examination of the ideas of four of

Lü's contemporaries—Hai Rui, Luo Rufang, Yang Dongming, and Feng Congwu; the second half, to a comparative analysis under four themes of the "exceptional" features in Lü's new outlook.

First, in the second half of the book, Handlin discusses Lü's distrust of human nature, which prompted him to take on the task of writing about "statecraft" (the method of managing the world). Lü considered human greed and selfishness only natural, and he proposed a remedy for these weaknesses not through "suppression" but through "accommodation." This pathological view of human society led Lü to assign a new identity, that of a doctor, to provincial officials. Lü's *Record of Practical Government* contains prescriptions for his patients.

Second, Handlin argues that Lü's pessimistic view did not deprive him of "the sense of mission as moral leader and educator," particularly toward the welfare of socially underprivileged people—women, children, and the poor. Unlike the popular lecturers of the Taizhou school, Lü communicated his ideas to his audience by writing didactic literature and handbooks in the vernacular language. Lü made an intriguing suggestion that "old blind female beggars be trained to perform songs from his *Precepts for Women* so that they could spread them to the female quarters of the elite households" (p. 149).

Third, the *Groaning Words* reveals how Lü, the "doctor," was as "ill" as the "petty men," lacking confidence in his own cure. Lü published *Groaning Words* to expose his inner frustrations. Handlin believes that Lü's preoccupation with illness and crises shaped his social thought.

Finally, Handlin evaluates Lü's "fact-centered" (as opposed to "ideal-centered") method of self-cultivation, while at the same time suggesting that "Lü had so little confidence in the efficacy of self-cultivation" (p. 169). Lü's method stressed the practice of keeping ledgers of merits and demerits in one's daily conduct. "The popularization of sagehood" through such a strenuous exercise "led to the growing need for statecraft rhetoric suitable for local government" (p. 212). Such a fact-centered outlook, Handlin concludes, was central in late Ming thought and, hence, signified a reorientation in connecting Confucian ideas to social challenges.

Handlin has written a brilliant book full of intellectual sparks. She has successfully imposed a clearly defined pattern on "the mosaic" of Lü's disconnected ideas and diverse themes. One major question, however, remains: Did the reorientation of Lü in the late Ming foreshadow the resurgence of interest in statecraft at the local level during the Ch'ing? Or might this resurgence have represented a revival of the central concern of Confucianism—which always stressed the link between human nature and efficient government—independently in response to fresh circumstances? Perhaps a defini-

tive answer can not easily be derived without a similar comparative study between Lü and his Ch'ing counterparts.

SILAS WU
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ROBERT MARKS. *Rural Revolution in South China: Peasants and the Making of History in Haifeng County, 1570-1930*. Madison: University of Wisconsin Press. 1984. Pp. xxv, 339. \$32.50.

Haifeng, a county in the East River region of the southern province of Kwangtung, the site of a soviet government in 1927-28, has attracted considerable attention from students of modern Chinese Communist history. To Roy Hofheinz, Jr.'s *The Broken Wave: The Chinese Communist Peasant Movement, 1922-1928* (1977) we can now add Robert Marks's provocative interpretation of the Haifeng revolutionary experiment and its historical antecedents.

Marks's monograph, a revision of his doctoral dissertation, is shaped by his desire to provide a Braudelian *longue durée* and to root the Haifeng revolutionary movement in the peasant experience itself. The peasants created their own world. In the seventeenth century they freed themselves from the manors that had dominated Ming agriculture and achieved conditions of permanent tenancy with encouragement from the early Ch'ing state. Traditional patterns of competition and conflict occurred among groups organized around lineage or market ties instead of along class lines. Both landlords and peasants in late imperial society shared a common set of assumptions concerning the moral economy.

The major stimulus preparing the ground for rural revolution came from imperialism, which transformed rural structures with increased commercialization of agriculture and modernization of textile production. The impact of the market collapse of Haifeng's major export, sugar, was exacerbated by the 1911 Revolution, which brought an urban reformist elite to power and spawned a new landlord class. This altered traditional tenancy arrangements and destroyed landlord-peasant solidarity. Class conflict entered the agenda in the decade after 1911.

Peng Pai, the son of a rich landlord family, organized peasant unions and became the "hero" of the Haifeng soviet. Marks rejects the conventional interpretation of Peng as the creator of China's peasant movement: the Haifeng movement belongs to the peasants themselves. He argues that Peng tried to superimpose a modern form of revolutionary organization (peasant unions) on a peasantry who saw him, instead, as a savior, "Peng the Bodhisattva." Peng was merely a "vessel" for transmission of peasant demands. The peasant movement ex-

pressed elements of the traditional peasant moral economy that had been abandoned by the new-style landlords. It was a form of collective action that grew out of local peasant culture.

This is an ambitious thesis. The author's argument on the historical background would have benefitted from acquaintance with the vast secondary literature in Japanese on Ming and early Ch'ing rural economy and tenancy. His characterization of the Kwangtung setting would likewise have been sharpened by recourse to anthropological literature on lineages in the Canton delta, where the seventeenth century brought not improved conditions for peasants but the creation of a few dominant lineages and many servile tenants. A sharper contrast between the environment of the delta and the East River region would have underlined the local history component of this study, which relies heavily on generalizations made for other regions or for the whole nation. At the same time, Marks's interpretation of the Haifeng peasant movement as a predominantly religious or cultic phenomenon should arouse lively debate and stimulate rethinking on the connections between traditional peasant culture and modern revolution.

EVELYN S. RAWSKI
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JANE HUNTER. *The Gospel of Gentility: American Women Missionaries in Turn-of-the-Century China*. New Haven: Yale University Press. 1984. Pp. xxi, 318. \$25.00.

Jane Hunter's book focuses on the community of American missionary women in China between 1900 and 1922. Hunter puts forward a meticulous account of both single and married women missionaries; she explains their visions of China, their community life (including missionary work), education, fashion, child-rearing, child-adoption, diets, views on nationalism, relationships with American male missionaries and Chinese men and women, and so on. Hunter also looks at the family backgrounds of these American women missionaries and their motivation for going to China. She attempts to place the life-styles and biases of these women in a sociohistorical context by first describing missionary activities in the U.S., the government policy of "civilizing" the East, the growing activism of American women, the changing political climate in China, and the escalating antimissionary sentiments among Chinese at the turn of the century. Toward the end of the book, Hunter evaluates the influence of these women missionaries on China and tries to compare them with Chinese Christian women.

The outstanding theme of this book is the defensiveness of American women missionaries in an

alien world—China. Instead of learning the philosophy and ways of life of the Chinese people, American women missionaries sought to preserve “Americanism” in their homes to the extent that they purchased canned food from the U.S. for consumption and updated their knowledge of the U.S. by reading fashion magazines. Furthermore, they ensured that their children were brought up in an “American” environment in China. These women did not appear to have much contact with the Chinese community except through their servants and a limited number of Chinese “clients” targeted for conversion.

Another theme that emerges from the book is the prevalence of sexism within the missionary world. Married women missionaries did not do much missionary work. They spent most of their time taking care of their children and their homes. The ideology of domesticity was promoted. It was only when their children had grown up that the women began to work in the missionary field.

Yet another theme not argued explicitly by the author is the predominance of racism and ethnocentrism among American women missionaries. Although they claimed to educate the Chinese, American women missionaries attempted to teach in English and not in Chinese. Few women missionaries learned the Chinese language. They also refused to use Chinese artifacts. They had a strong suspicion of Chinese nurses and servants and were worried lest these domestics “contaminate” the minds of their children. They had a stereotypical image of Chinese men as ladylike and weak. Ironically, it was the Chinese servants who, in the analysis of the author, “liberated” women to do their missionary work. In the views and practice of these American women missionaries, however, Chinese people were seen as second-class human beings who were to be uplifted by Christianity. A strong mixture of cultural chauvinism and racism permeated the missionary world.

While Hunter's book provides great insight into the little-known world of American missionary women, its methodology and presentation are problematic. Hunter's data came mainly from the letters and private papers of forty women connected with the Congregational and Methodist Episcopal missions, from a few publications by Chinese women, and from interviews with a few women. Given this type of information, it is only logical that the author would see the views, feelings, concerns, and expectations of American women missionaries as providing an introspective explanation of events. By focusing on personal feelings, the author often loses sight of structural explanations. Although Hunter has two chapters on the larger sociological context, she fails to link the changing social structure with the themes of the book—defensiveness of “American-

ism,” sexism, racism, and ethnocentrism. Furthermore, correspondence with relatives and friends tends to glorify or simplify events or occurrences. Therefore, Hunter's book, which is based largely on correspondence, does not go beyond a superficial level of understanding of historical events.

The last point is also obvious in Hunter's presentation of materials in the book. Very often, rather than undertaking deeper analysis, she jumps from one topic to another without trying to link them together. Although the readers are delighted to read some of her findings, they are often deprived of coherent analysis. The last two chapters appear to have little relationship to the rest of the book. Arguments remain disjointed, and some work needs to be done in linking the liberation of women with the American women missionaries.

BOBBY SIU

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SHIH-SHAN HENRY TSAI. *China and the Overseas Chinese in the United States, 1868–1911*. Fayetteville: University of Arkansas Press. 1983. Pp. ix, 166. \$17.50.

The attitudes and motives behind the anti-Chinese movement in the United States have been well researched by historians such as Gunther Barth, Stuart Creighton Miller, and Alexander P. Saxton. The other side of the question, however—the responses of the Chinese government and its officials in the United States to discrimination and immigration restriction of the overseas Chinese—has not merited much attention. In his recent monograph Shih-shan Henry Tsai carefully explores the evolving attitudes and policies of the Chinese government toward the issues of racial prejudice, violence, and immigrant exclusion.

Not accustomed to dealing with the West, the Ch'ing government practiced a multifaceted diplomacy on three different levels: through its *Tsungli Yamen*, a fledgling foreign ministry; the governors-general in the provinces; and the Chinese envoys stationed abroad. Because of the decentralized nature of its diplomatic apparatus, the Chinese conduct of foreign affairs often proved baffling to Western diplomats. At the same time, the Ch'ing government was successively hostile, indifferent, and solicitous about the welfare of the overseas Chinese. Initially fearing resistance and subversion from Ming loyalists, the Ch'ing authorities had banned foreign emigration in 1644. Not until the Burlingame treaty of 1868 was there provision for the appointment of consuls to American ports for the protection of Chinese nationals. Even then, the first Chinese minister to the United States, Ch'en Lan-pin, did not arrive until 1878.

For all their representations against racial violence and treaty violations, Chinese diplomats in

America found that they were compromised by the weakness of their nation. Not to be disregarded, either, was the fact that U.S. officials in Washington were often more concerned with electoral considerations and sectional politics than with the protests of Chinese ministers. Increasing frustration led Ch'ing officials to change their strategy from one of protest to a self-prohibitory policy curtailing the emigration of laborers. At another level, widespread anger and resentment toward the discriminatory treatment accorded their fellow countrymen finally erupted in a Chinese boycott in 1905 against American goods. While some Ch'ing officials sympathized with this spontaneous and nationalistic outburst, others feared that such sentiment might later be redirected against the Manchu dynasty itself. As a result, the Ch'ing government eventually adopted a policy that successfully arrested the boycott movement.

In this slender volume, Tsai has produced a useful summary of Ch'ing governmental responses to discrimination and immigration restriction against the Chinese in the United States. Drawing on the diaries of Chinese ministers such as Ch'en, Chang Yin-huan, and Ts'ui Kuo-yin, as well as unpublished materials from the Academia Sinica Archives in Taiwan, he was able to chronicle the opinions of the different diplomats. More should probably have been said about the divergence in views between the Chinatown communities and the Ch'ing officials, but that does not detract from the value of his work. Together with Michael H. Hunt, Tsai has integrated diplomatic and immigration history to enhance our understanding of Sino-American relations.

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PAUL A. COHEN. *Discovering History in China: American Historical Writing on the Recent Chinese Past*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1984. Pp. xviii, 243. \$25.00.

Every historian of China should read this book, for what Paul A. Cohen has done here is to lay bare the hidden assumptions that have informed and skewed much American research on nineteenth- and twentieth-century China. He shows that the questions most American historians have asked about the Chinese past, and consequently the kind of histories they have written, have been determined as much by their own cultural biases as by the historical realities of China itself.

Cohen has authored two books that focused on the impact of Western culture on China in the late nineteenth century and on China's response to that

impact. During the late 1960s and early 1970s, however, while writing his second book, he became uneasy about his premise that the central questions in China's modern history were related to how China responded to the Western impact. Not until after publishing the book, however, did he seriously attempt to come to grips with the premises that underlay his own work and that of most other American historians of China. This book is the result of that reexamination.

Chinese historical studies in the United States became a professional enterprise, wherein researchers were schooled in Asian languages and accorded access to important Chinese source materials, only after World War II. But, in Cohen's view, the professionals of the 1950s and 1960s, like the pre-war "amateurs" who had written on China, continued to think that "traditional" China was a somnolent, unchanging society and that progressive change was introduced to China only after the dynamic and "modern" Westerners forced themselves onto Chinese soil and into the Chinese consciousness.

Possessed of this intellectual construct, American historians of China naturally conceived that the really important questions to be asked about China's recent past related to the shaping role of the West in Chinese affairs and that the principal story line in Chinese history was China's development from a state of traditional agrarian backwardness to a state of Euro-American-style modernity. By critically examining much of the most influential literature in the field, Cohen suggests that three analytical paradigms have characterized America's research on modern China: (1) the "impact-response paradigm," which focused on the impact of a dynamic West on a passive and reactive China; (2) the "tradition-modernity paradigm," which assumed that China was teleologically determined to progress from traditional agrarianism to Western-style industrialization; and (3) the "imperialist paradigm," which premised that imperialism was the causative force (for good or ill) in China's economic, political, and social development.

The problem with the studies that fit these paradigms, Cohen argues, is not that the role of the West in Chinese affairs was unimportant but that these studies frequently overlooked endogenous change or attributed endogenous changes largely to foreign influences. American studies of China in the 1950s and 1960s, that is, were overwhelmingly Western-centric and seriously distorted Chinese historical realities.

Cohen does not merely criticize, however, and in a final chapter he prescribes a "China-centered approach" to the study of Chinese history. Such an approach would (1) adopt Chinese rather than Western criteria to decide what is significant in

China's past; (2) avoid gross generalizations about China as a whole, and instead disaggregate China geographically by encouraging local and regional histories; (3) disaggregate China socially by examining lower as well as elite strata of society; and (4) strive for interdisciplinary analysis, largely by incorporating social-science theories and techniques.

Reading Cohen's book is a consciousness-raising experience. Whether or not readers agree with his critiques of individual authors, they will assuredly derive from the book a heightened awareness of the dangers of ethnocentric distortion.

LLOYD E. EASTMAN
University of Illinois

JOHN J. STEPHAN. *Hawaii under the Rising Sun: Japan's Plans for Conquest after Pearl Harbor*. Honolulu: University of Hawaii Press. 1984. \$16.95.

The concluding chapter of this fine monograph is entitled "The Persistence of Illusion." The chapter is a fitting climax to a disquieting book, which shatters several historical illusions that have almost come to be accepted as facts. It will remind historians how complex and ambiguous history really is.

John J. Stephan has explored his topic with great bibliographical ingenuity, utilizing many unpublished and other obscure materials in both Japanese and English. He finds that, contrary to standard impressions, the Japanese military *did* make plans to seize and occupy Hawaii in the aftermath of Pearl Harbor. "Less than forty-eight hours after the attack on Pearl Harbor," Stephan begins his narrative, Combined Fleet Admiral Isoroku Yamamoto "ordered his staff to prepare plans for an invasion of Hawaii" (p. 1). The Japanese army, despite its orientation toward continental Asia, gradually came to endorse an "Eastern Operation" plan for taking Hawaii, until June 1942 when the naval defeat at Midway ended its practical prospects.

Even after that, Japanese civilian agencies, both private and governmental, building on a long tradition of assumptions about the "Japanese-ness" of Hawaii and its Japanese immigrant inhabitants, planned ways and means of including Hawaii in Japan's "Greater East Asia Co-Prosperity Sphere." While journalists and scholars spoke and wrote of the coming "liberation" of Hawaii, several government agencies included plans for the political, economic, and social reconstruction of Hawaii in their agendas for the future.

Japanese planners assumed that Hawaii's "Japanese" population would support them. Stephan provides an excellent resumé of the process of Japanese immigration to Hawaii and then goes into considerable detail concerning relationships between the home country and what Japanese officials called

dōhō (compatriots). He notes that "an intricate web of human ties bound Hawaii to both sides of the Pacific" (p. 7) and that, despite various attempts to stereotype Japanese Americans either as "vulnerable to penetration by spies and saboteurs" (the wartime stereotype) or as "100 percent American" (the stereotype that superseded this), the record shows ambivalence. Although the (American) patriotism of the 442nd Division of *nisei* (second-generation) volunteers was strong indeed, the record also shows that the *issei* (first-generation) immigrants to Hawaii were generally sympathetic to Japan and hoped for a Japanese victory.

This attitude had its roots in the first-generation immigrants' strong favoring of Japan over China in the tensions and war between those two countries in the 1930s. Hawaii's Japanese-language newspapers called Japan's army "our army" and its victories "our victories." Although most *issei* were shocked and distressed at the Pearl Harbor attack, they continued to make excuses for the homeland. Many had visited Japan, contributed to Japanese anti-China causes, and continued to feel Japan's cause was just and its victory likely, some even refusing to believe Japan had been defeated in 1945. *Nisei* were generally pro-American but felt ambivalence not only about their parents but also, in some cases, about their citizenship. Although American citizens by birth, very few had renounced their Japanese citizenship by right of *jus sanguinis*, which was automatically theirs under Japanese law until 1924 and after that date if they were registered by their parents at the Japanese consulate. Thus, Stephan concludes, their situation was "full of complex nuances and ambiguities," and "both Japanese and Americans have reason to be thankful that the islands were not seized in 1942 or 1943" and that Hawaii escaped "the scars left by combat and collaboration" (pp. 176-77).

HILARY CONROY
University of Pennsylvania

VEENA TALWAR OLDENBURG. *The Making of Colonial Lucknow, 1856-1877*. Princeton: Princeton University Press. 1984. Pp. xxv, 287. \$32.50.

This is a splendid book and should be read by everyone interested in the nineteenth-century *raj*, especially by graduate students entering the field. Veena Talwar Oldenburg has avoided all of the stigmata of a revised dissertation; her work is graciously written, well organized, and thoroughly researched. One of the great strengths of the study is its general approach: the city of Lucknow is appraised as a battlefield in the struggle over security, order, public health, taxation, and loyalty. One meets in the footnotes old friends, such as the

traveler Fanny Parkes, the historian J. W. Kaye, the correspondent W. H. Russell, as well as contemporary scholars such as Bernard Cohn and Kenneth Ballhatchet.

The most interesting chapter is on municipal taxation. The people of Lucknow had not experienced direct taxation under the nawabs. Only once, in 1858, a tax was imposed on "disloyal" elements; the author clearly explains how unfair this tax was. Subsequently, a regular tax called the octroi was levied on all goods entering the city. The octroi replaced the *chung*, a kind of sales tax placed on goods whether or not they were sold. If the goods were sent to another town, they were liable to duty all over again. Not only was this tax difficult to collect in a city without walls, but it was also administered by a corrupt body of officials. The author presents clearly the difficulties of tax assessment, including the painful compilation of the first tax registers. The tax was levied on even the poorest artisans, because an income of more than 2.8 rupees a month was taxable. It is rather quaint that no women appeared on the tax registers except prostitutes, apparently because prostitution was the only profession for women thought profitable enough to be taxed. The registers were conscientiously updated and used for many official purposes; jury lists, for example, were drawn from a roll of the biggest taxpayers in the city. One of the most significant results of this efficient taxation was that tradesmen began to keep a second set of fictitious books designed for the tax assessor. In the end Lucknow became a model for cities where, as the colonial presence was expanded, the city was kept relatively clean and safe by the expenditure of large sums of money. The people were angered by the heavy-handed collection, but the majority had no choice but to pay. Those who protested were punished for their temerity.

Perhaps the weakest chapter is on public health. More information is available on cholera, typhoid, smallpox, plague, and malaria than Oldenburg has broached. Other cities had similar problems. The technical (medical) difficulties here are great, but there is no evidence that the author has wrestled with them. In cleaning up their environment for self-protection, the British had to provide much general sanitation, since diseases do not respect social class. The author recognizes this problem, yet it is not clearly resolved.

Unfortunately, the legacy of government left by the local elite in Lucknow was inappropriate to meet contemporary problems. The sahibs invented a system that worked well for them. Obviously they did not prescribe for present problems of overpopulation, unemployment, and scarce housing. It seems

unfair to say that the makers of colonial Lucknow were fundamentally wrong.

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B. J. TERWIEL. *A History of Modern Thailand, 1767-1942*. (Histories of Southeast Asia Series.) New York: University of Queensland Press. 1983. Pp. x, 379. Cloth \$29.95, paper \$17.95.

There are formidable obstacles in the way of any who would write the history of Thailand. Some are inherent in the sources, some are imposed by received wisdom so long ingrained as to become habitual thinking, and some are created by the historian in defining a topic or period or geographical unit.

B. J. Terwiel's aim is modest—"to provide a readable historical narrative." In pursuing it he acknowledges the influence of "the Siamese chroniclers, who present events in chronological order and provide information from various perspectives" (p. ix). He begins with a sketch of "Society during the Late-Ayutthaya Period" and then devotes a chapter each to Taksin and the first seven kings of the Chakri dynasty (two to Chulalongkorn), concluding with the rise of the military regime and the onset of World War II in Thailand.

This is both a tantalizing and a frustrating volume—tantalizing because nearly every chapter contains scattered paragraphs on subjects ignored by other writers and frustrating because these paragraphs do not amount to anything significant. The first chapter, for example, contains a thoughtful analysis of the form and workings of the *sakdina* ("dignity-mark," numerical-ranking) social and administrative order of the *ancien régime* but little that could afford the reader an appreciation of the nature of "society," of values and behavior, in old Siam. Chapter 2 is excellent on the details of King Taksin's rise to power in 1767-69 and includes a stirring defense of Taksin against those who dismiss him as mad or megalomaniacal, but here (and elsewhere) there is no sense of politics, contending interests, or economics. The chapter on King Vajiravudh (1910-25) makes much of the king's "nationalism" without suggesting any of the social and political dimensions of that word: "patriotism" would have done as well. Terwiel has much to say that is new, but, throughout, his treatment is governed more by the exigencies of chronological sequence than by the application of any vision the author might have of the shape, the form, and the dynamics of Thailand's history over the past two centuries. Nowhere is this more apparent than in the book's abrupt ending with the events of December 8, 1941. The book's final sentence reads, "As he

had already indicated in November 1940, Phibun-songkhram had opted for Japan's 'New Order', hoping that the Siamese would be a leading force in the new configuration being created" (p. 346). This is neither a conclusion nor an ending. Terwiel may have made some progress in mining intractable sources and challenging received wisdom, but by failing to define either "Thailand" or its "history" he has not left us much better off than we were before he wrote this book.

DAVID K. WYATT
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JURRIEN VAN GOOR. *Kooplieden, predikanten en bestuurders overzee: Beeldvorming en plattsbepaling in een andere wereld* [Merchants, Preachers, and Administrators Overseas: The Forming of Images and the Finding of Positions in Another World]. (HES Studies in Colonial and Non-European History.) Utrecht, Netherlands: HES Uitgevers. 1982. Pp. 212. f 39.50.

This work is composed of four distinct essays, each in its own manner contributing to our knowledge of various milieus in southern Asia from the seventeenth to the nineteenth centuries. At first glance, the sole unifying theme would seem to lie in its title. On closer inspection, it becomes clear that the essays' widespread temporal and geographical settings stem from Jurrien Van Goor's choice of source material. Extensive Dutch activities in Asia during this period resulted in the production of voluminous historical observations that were preserved in the official records of the Dutch East India Company. As the work demonstrates, these sources can be used to investigate a variety of specific times and places.

Particularly illustrative of this is the saga of the Greek merchant-adventurer Constant Phaulkon (chapter 1). Originally coming to Asia with the English East India Company, Phaulkon entered the service of the Siamese king in 1680 and through undoubted abilities rose to become a trusted adviser on matters dealing with trade and foreign merchants. In this capacity, Phaulkon was in a position to further or obstruct competing Asian and European mercantile interests in Siam. Thus, it is not surprising that his rise and ultimate fall from power in 1688 are conscientiously recorded in the Dutch materials.

A somewhat more original contribution is found in "Preachers in the Hindu-Buddhist World" (chapter 3), here defined as Negapatnam and Ceylon during (respectively) the late seventeenth and early eighteenth centuries. In particular, the chapter focuses on the confrontation between the Calvinistic doctrines specifically recognized as constituting the

only true belief in territories controlled by the Dutch East India Company and the realities of the Hinduism and Buddhism practiced by the overwhelming majority of the local populations. Van Goor dramatizes the confrontation by relating the difficulties encountered by Dutch preachers in the service of God and the company.

Interest in the remaining chapters, which discuss the almost predictable conflicts between Dutch administrators and Indonesian potentates, lies mainly in the wealth of detail presented. Introduction is provided by chapter 2, which deals ostensibly with Said Abdullah bin Abdulkadir Jelani, a Surabayan of Arabic ancestry who served the ruler of Lombok during the 1880s as a *shahbandar*, or port official. In actual fact, it relates the expansion of the Dutch into the Bali-Lombok region, an expansion accounted for by the strong European personalities of the time, although some reference is made to the socioeconomic stage on which the actors played.

In the final essay (chapter 4), the author attempts to come to grips with the broader historical problem of imperialism in the Indonesian context. In so doing, he borrows concepts from the scholarly literature on the subject. These include, in addition to "imperialism" itself, the assumption of the superiority of European governmental systems over those of the local potentates and the corollary that the former must be imposed in the region in order to protect the "common man" from exploitation by the latter. These provide points of departure for a somewhat diffuse discussion of Dutch power in the archipelago. As the author rightly points out, such an attempt is made all the more difficult by a lack of serious study of the precolonial systems. The result is a concentration on various case studies of the formative stage in Dutch imperialism, specific examples being taken from Madura, Bali-Lombok, and Aceh during the late nineteenth century.

A few characteristics of the author's approach to history may be noted by way of conclusion. Most striking in this regard is the proposition that the superficial similarity between the time of Phaulkon and that of Abdullah, separated by some two centuries, supports "the presupposition that the innate character of the Southeast Asian world continued to exist until well into the nineteenth century" (p. 16). This modern version of "the unchanging Orient" of the "ageless Indies" is at best nostalgic. Modern scholarship is, on the contrary, impressed by the dynamic nature of the region's response—albeit forced—to the presence of Europeans at this time.

A second significant characteristic is the author's view of historical causation. Although the work formally deals only with the picture of the overseas world provided by "merchants, preachers and administrators," its contents leave no doubt that it is the "great men" who move history. This also may

account for the somewhat rambling nature of the essays, which are more interested in relating all of what the actors said or did than in setting them in some sort of interactive relationship through which one could explain events.

The final point concerns the source material—not only the inconsistent manner of citing primary sources but also, and more importantly, Van Goor's definite Dutch-centric approach (from the perspective of the East India Company or the East Indies government). At times this provides a valuable corrective to overreliance on other European sources; at times it is a methodological necessity because of a lack of sufficient local historical materials. At all times, however, it must be remembered that this is a Dutch view based on Dutch sources of primarily Dutch activities in Asia. As such, it admirably contributes to the literature on the expansion of Europe overseas.

MASON C. HOADLEY
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JEAN GELMAN TAYLOR. *The Social World of Batavia: European and Eurasian in Dutch Asia*. Madison: University of Wisconsin Press. 1983. Pp. xxii, 249. \$25.00.

The factories of the Dutch East India Company (VOC) were located from the Cape of Good Hope to Japan. At each of these posts, European men in the service of the company lived with local women and begot children. In this way, a sort of company mestizo society was formed for which Batavia, after its founding in 1619, became the major center of company activity. In Batavia, the mestizo colonial society had its longest significant period of development and assumed the characteristics sketched in this book. Eurasian women became the fixed or settled element in this society, and some of them became extremely wealthy, since more than one husband would provide them with property. These women married men from Europe whose rank in the VOC hierarchy they deemed sufficiently worthy. Around such marriage arrangements a life of seignorial splendor developed in which company position, family connections, and inherited wealth were the keys to rank and status.

Jean Gelman Taylor devotes about two-thirds of this book to the VOC period (until about 1800) during which this society developed its self-sustaining form. She describes with verve and insight the marriage patterns, economic connections, and religious and social life of Batavia's elite. For this VOC period she makes extensive use of genealogical tables and family linkages that are detailed further in the three appendixes. This portion strikes me as the best-supported and most cogently argued part

of the book. It also presents some interesting hypotheses about the rejection of European cultural styles and the inability of the VOC directors to effect changes in the Indies life-style. It presents only a limited picture of Batavian society, however, for its concern is chiefly with the social elite of wealth and property. There were also a lot of poor Eurasians in Batavia in these years, to say nothing of Chinese and various Indonesian groups who made their life and livelihood in the city. None of these groups, however, is touched on in this book. It may not have been the author's intent to give such a rounded social picture, but, without some effort to do so, the upper-crust mestizo elite is left floating in space.

The final third of the book seeks to describe the fate of this mestizo society during the nineteenth century, when Dutch and, for a brief period, English colonial administration replaced the mercantilistic company. The modern administrators had visions and plans, which did not include the old company elite nor their life-style. Yet, the mestizo ladies were still present and the young administrators, with very few exceptions, came out to the East as bachelors. Naturally enough, they married (or contracted other sorts of liaisons with) these women, and again an elite of high government functionaries appeared. In the nineteenth century, however, there was a greater infusion of European culture and a more positive effort toward modifying the mestizo life-style, since most of the young European administrators did not intend to live out their lives in Java. These modifications took the form of education and social graces, which gave a European veneer to the Indies life-style of the verandah, the sarong, and the kebaya. In the first half of the nineteenth century a few English and Dutch women sought to upgrade these patterns of behavior, but they had only a passing influence. The mestizo patterns prevailed in Java, and not just in Batavia, during the Dutch colonial period to 1942. These patterns, however, were increasingly driven to modification and were continuously downgraded as a greater number of women came from Europe after the late nineteenth century.

In addition to family histories, this portion of the book also draws on newspaper advertisements, government regulations, and novels written by colonial women. Although some interesting sociological insights are provided in this part of the book, I find its transitions unclear and its timing of changes somewhat dubious. This is partly because of the nature of the sources and partly to the growing complexity of a society that was no longer just limited to Batavia but spread out over the entire island. In attempting to compress these complexities in the latter portion of the book, Taylor tries to do too much in too little space and with too little material. It might have been

better to end the book in 1800 and to provide more details and illustrations from the VOC period.

ROBERT VAN NIEL
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K. S. INGLIS. *This Is the ABC: The Australian Broadcasting Commission, 1932–1983*. Assisted by JAN BRAZIER. Melbourne: Melbourne University Press. 1983. Pp. ix, 521.

This study was commissioned by the Australian Broadcasting Commission (ABC) to celebrate its fiftieth anniversary. K. S. Inglis's sympathetic narrative describes the establishment of the ABC by Parliament in 1931 to provide "adequate and comprehensive" (p. 18) national radio programs; the commission's contribution to Australia's war effort; the subsequent extension of its services into overseas broadcasting, rural radio, and parliamentary broadcasting; the creation of its own news-gathering agency; the belated introduction of national television in 1956; the attempt in the later 1960s to increase its appeal through current affairs programs; and the ABC's expansion under Labor Prime Minister Gough Whitlam (1972–75), which was accompanied by internal division and followed by retrenchment. With an annual budget of £150 million, six thousand employees, and a mandate to "inform, entertain and educate," the ABC in 1981 was, according to the Dix Committee of Review, "slow moving, overgrown, complacent, and uncertain of the direction in which it was heading" (*Sydney Morning Herald*, May 3, 1984).

Notwithstanding its lackluster performance, the commission survived relatively unscathed for three reasons. First, the Australian Labor party, which was responsible for four of the five acts providing for the ABC's existence, saw it as a means for raising the cultural standards of the masses through high-quality radio programs featuring orchestral music, drama, religious services, childrens' sessions ("for the training of young Australians in the ideals of good citizenship and the enjoyment of the beauties of life and literature" [p. 56]), sporting commentaries (especially on cricket), news bulletins, and informative but uncontroversial talks. After the war, political considerations led to its expansion. Joseph B. Chifley's Labor government authorized the ABC to develop a news-gathering service so that it would not have to depend on tainted information from capitalist sources; parliamentary broadcasting was introduced "to raise the standard of debate . . . and contribute to a better informed judgement throughout the country" (p. 128); and, under Whitlam, radio was perceived as an instrument that could be used to advance the cause of political reform. Second, the staff of the ABC staunchly supported the Labor party's mission of enlightenment. From the

outset, the organization was "a thoroughly imperial artifact" (p. 19), modeled on Britain's BBC. Its well-bred bureaucrats were men with a high sense of moral purpose for whom the indulgence of middle-class taste was synonymous with the elevation of working-class taste. Third, although the ABC, unlike the BBC, was never granted a monopoly to pursue its mission, its existence did not depend on its ability to hold audiences. The ABC's income, derived initially from license fees and after 1948 from consolidated revenue, was assured, even though it attracted only 20 percent of the national audience. So, apart from a brief interlude in wartime, hard-line highbrows had no difficulty retaining control until the mid-1960s. Subsequent attempts to chase the ratings proved unsuccessful, except in current affairs and FM radio, where the Young Turks who flouted traditional attitudes and procedures clashed with senior managers, who shrank from alienating the ABC's faithful supporters.

However hard the commission tried not to offend, it was inevitable that the ABC should be subject to political interference at times, to an extent that cast doubt on its vaunted autonomy. The wartime Labor government was "in no state of mind to show a nice respect for the independence of a statutory authority" (p. 95), and the ABC's independence was subsequently circumscribed by legislation and directives, by a drive against communists, by ministerial vetoes on politically embarrassing programs (which commercial stations promptly put out), by accusations of political bias, by the efforts of politicians to obtain publicity, and by the politicization of top appointments. A substantial part of Inglis's story is therefore concerned with the passionate controversies and meaningless compromises of chairmen and general managers, the details of organizational change, and the gradual deterioration in industrial relations.

The study has several limitations. Inglis rarely touches on financial and economic matters. The external environment in which the ABC operated (for example, the performance of the ABC's commercial competitors and the change in social values that occurred in the 1960s when the ABC began to chase the ratings) receives little attention. Why did the same television programs put out by the ABC attract larger audiences as repeats on commercial networks? Was outside interference excessive compared with the experience of commercial stations or the BBC?

Despite too much minutiae in parts, Inglis has made a significant contribution to recent Australian history. His study sheds light on middle-class Australian values (one chairman thought it "unseemly to allow royal speech to go out on frequencies adulterated by advertising" [p. 33], and a minister sought to

have a news reader fired "for having an anti-Labor voice" [p. 99]), on the stifling effects of an unwieldy bureaucracy, and on the promotion of an institution designed to foster a sense of national identity.

GORDON RIMMER
University of New South Wales

FRANCIS X. HEZEL. *The First Taint of Civilization: A History of the Caroline and Marshall Islands in Pre-Colonial Days, 1521-1885*. (Pacific Islands Monograph Series, number 1.) Honolulu: Pacific Islands Studies Program, Center for Pacific and Asian Studies, University of Hawaii. 1983. Pp. xvi, 365. \$25.00.

Francis X. Hezel has written a lively history of the North Pacific Caroline and Marshall islands prior to colonial rule. It tells the story of contacts between Europeans and islanders from the first Spanish encounters through the early failure to spread Christianity and the rediscovery of the islands by British, French, and Russian explorers to the establishment of permanent European contacts by beach-combers, traders, and missionaries.

The account is enlivened by some delightful descriptions. We read of Paulau islanders "wearing nothing but their tatoos," stroking the arms of seamen of the *Antelope* "to determine where their garments ended and the skin began" (p. 134). And we observe the American missionary, Benjamin Snow, training his spyglass on ships in port at Kosrae, keeping "a sharp lookout for women smuggled onto whaleships, later confronting the unsuspecting captains who called on him with a recital of their sins, often in surprisingly accurate detail" (p. 162).

The book is thickly populated by European intruders ranging from scientific investigators to brutal escaped convicts. Some are notorious, such as the pirate Bully Hayes and the trading tsar David Dean O'Keefe. Others have been rescued from historical obscurity. Some island leaders are also brought to life, such as Chief Jubelick on Arno in the Marshall islands "wearing a distinctive red turban and a 'brightly coloured handkerchief' over his native grass skirt" and darting backward and forward behind stone fortifications, "rallying his three hundred fighting men to keep up the rifle fire" (p. 294).

The author is a Jesuit priest who has long been a teacher on Truk atoll and who in previous articles has pioneered the history of his island region. He pays careful attention to comprehensive and accurate historical detail, whether it be accounts of violent clashes between islanders and Europeans or the operations of trading companies. The book is well documented and based on a wide range of primary sources. It has only a few historical errors, such as predating by six years the end of the Pacific islands' cotton boom after the American Civil War.

In revealing for the first time the history of these Micronesian islands, the book also assesses the effects of European influences on the islanders. It demonstrates the familiar destructiveness of "blessings" of civilization, such as disease, grog, and prostitution. Hezel also reinforces studies of cultural contacts elsewhere in the Pacific, which have modified the traditional story. Guns, for instance, did not increase deaths in tribal warfare but had the reverse effect, because they kept enemies at safer distances from each other. And the islanders showed considerable discrimination in their borrowings from Europeans. They accepted, for example, Christianity and some of its by-products, like Mother Hubbard dresses. But they rejected missionary pressures to give up kava drinking or democratize their social institutions.

This book certainly deserves a prominent place on Pacific history bookshelves. Added bonuses are well-drawn maps and a comprehensive index. The publishers are to be congratulated for the first volume in their new Pacific Island Monograph Series.

ROGER C. THOMPSON
University of New South Wales

K. R. HOWE. *Where the Waves Fall: A New South Sea Islands History from First Settlement to Colonial Rule*. (Pacific Islands Monograph Series, number 2.) Honolulu: University of Hawaii Press. 1984. Pp. xix, 403. \$29.95.

In the last twenty-five years Pacific history has emerged as a disciplinary specialty, in which a growing group of scholars provides new interpretations of the islanders' past. One of these, K. R. Howe, attempts in this book to give the lay reader a "state of the art" overview of "modern Pacific history" (p. xvi).

The "South Sea Islands" of the subtitle are those found in Polynesia and island Melanesia. In fact, the book's coverage is less even than Howe implies. More than 40 percent of the text deals with six island groups. Here Howe argues that centralized kingdoms arose in the early nineteenth century in Tahiti, Hawaii, and Tonga because those islanders had developed highly stratified societies before significant European contact. In contrast, less hierarchical societies in New Zealand, Samoa, and Fiji were incapable of using alien technology and personnel to establish rulers like Pomare, Kamehameha, and Tafa'ahu.

This argument almost constitutes a book within a book. Howe adapts categories developed by anthropologist Irving Goldman in the latter's analysis of traditional Polynesian society to advance his own interpretation—an example of interdisciplinary approaches characteristic of "modern Pacific history."

Sections 3 and 4 of the book provide the most detailed exposition of Howe's basic "revisionist" position: Pacific island societies were neither static nor passive, and their history must be understood in terms of the dynamic interplay of both indigenous and foreign cultural forces.

In contrast to the extended discussion of these six polities, the following section on Melanesia appears rather sketchy. Perhaps this is a consequence of the book's time frame, since significant European impact on these islands began after the important changes already described for those to the east. The "book within a book" also makes the first two sections seem less substantial by comparison. Time lags in publication mean that the ever-accelerating growth of scholarship on Pacific island prehistory cannot be fully treated, but within these limitations Howe gives his lay reader a useful synthesis. The second section on early exploration, trade, and missionary activity is similarly effective.

By addressing the book to the nonspecialist, abandoning claims to comprehensiveness, and presenting his work as one of several possible interpretations, Howe has done much to disarm the critic. Specialists will certainly find many contentious points, but only one need be raised here. No one could quarrel with Howe's underlying premise: that islanders were active participants in creating their own history. He goes too far, however, in his attack on writers in the "fatal impact" tradition. Reading this final section describing the vitality of island society, one could easily forget that Tahitians and New Caledonians are still subjects of a foreign power, while Maoris and Hawaiians are found in disproportionate numbers at lower social strata on their respective islands. Surely this epilogue might have noted the significant differences between traditional and modern systems of Pacific social stratification.

Nevertheless, *Where the Waves Fall* is a worthwhile offering to the interested nonspecialist. The book is very well produced. Typographical errors are minimal, maps are satisfactory, and well-chosen illustrations and quotations from earlier works enhance the volume's attractiveness. A cheaper paperbound edition would be useful for undergraduate courses on Pacific history.

EUGENE OGAN
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UNITED STATES

RICHARD WEISMAN. *Witchcraft, Magic, and Religion in 17th-Century Massachusetts*. Amherst: University of Massachusetts Press. 1984. Pp. xiv, 267. \$22.50.

Yet another book on New England witchcraft? Yes, but with a significant twist. Where other recent

studies probe New England town disputes, Puritan psychology, or underground occultism as the source of New England's trials, Richard Weisman turns directly to their legal and theological foundations in an informative but also flawed analysis.

Following Keith Thomas and Alan Macfarlane, Weisman argues that two different traditions underwrote Bay Colony witch trials. Before Salem, most accused witches came to the attention of magistrates through lay complaints rooted in popular conceptions of magic and witchcraft. These complaints stressed the evil of using occult means to harm people and things—"maleficium." When Massachusetts magistrates examined the accused, however, they proceeded with quite different theologically informed conceptualizations that stressed the evil inherent in witch covenants with the Devil. Amid this shift, and before 1692, local magistrates and higher courts acquitted many defendants and executed few convicted witches. Yet the court proceedings still soothed local tension because they effectively labeled the accused as social deviants, if not as witches.

A double turnaround occurred in 1692. Weisman argues that the specially appointed court (composed of Boston, not Essex County, magistrates) no longer merely adjudicated popular witch complaints but became a major accusatory agent that also executed the many defendants it convicted. In turn, this judicial carnage shattered the old, awkward synthesis. The argument about "spectral" evidence revealed a dangerous magistrate-minister conflict over authority in the colony; it exposed the intellectual contradictions in lay and theological definitions of witchcraft; and its resolution—the theological definition triumphed as authorities denigrated lay concern about maleficium—ironically precluded future prosecutions and produced the famous apologies from Judge Samuel Sewall and, later, the Massachusetts General Court.

Weisman's analysis clarifies several interesting problems. If he necessarily follows John Demos in underscoring the perverse social utility of even the pre-1692 acquittals, he quite rightly stresses the prosecutions' deeply bifurcated intellectual foundations. These always were unstable, and Salem's reversals of the old prosecutorial patterns left everyone unsatisfied. Weisman notes that amid the rising concern for Salem's convicted witches some local residents still criticized authorities for stopping the trials because so many witchcraft allegations remained unresolved.

All the same, a methodological narrowness compromises the book. The exclusive focus on Massachusetts is difficult to justify given the colony's sparse court records and the regional context of the witch trials, and the failure to draw Connecticut into the analysis confuses attempts to compare the book

to the many broader New England witchcraft studies. The analysis of the legal and theological traditions underwriting the Massachusetts prosecutions similarly demands wider horizons. As Samuel Eliot Morison and Perry Miller argued long ago, New Englanders inhabited a more expansive intellectual universe than the "Puritan" label—and this Massachusetts study—suggest. These limitations point up the desirability of broader, more comparative approaches to the subject, just as Weisman's recovery of hidden social and intellectual dynamics in the Massachusetts witch trials tells us why historians will continue to pursue this ever elusive prey.

JON BUTLER
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JESSICA KROSS. *The Evolution of an American Town: Newtown, New York, 1642–1775*. Philadelphia: Temple University Press. 1983. Pp. xviii, 335. \$25.00.

For a couple of reasons we should welcome Jessica Kross's study on Newtown, New York, 1642–1775. It is the first of the published local studies on a New York colonial agrarian community, and, together with the works of James T. Lemon, Stephanie Wolf, and others, it helps break the monotonous New England mold and perspectives that have so long dominated colonial local history research. But this study of Newtown does not contribute to the trend in early American history toward fragmentation, chaos, and confusion that Bernard Bailyn warned about some time ago. Kross has taken pains to avoid the parochial outlook of her predecessors by placing her subject in the comparative framework of other town studies. Her approach is synthetic; it has the virtue of telling us about the major aspects of Newtown's life that were either typical or unique in colonial society. Yet this synthesis has not been achieved at the expense of an exhaustive search and research of primary sources.

Kross's principal objective is to analyze the political, economic, social, and legal changes that Newtown (about sixteen thousand acres) experienced from its founding by Englishmen from Connecticut in 1642 to the American Revolution. She traces the changes through four chronological periods: 1642–64, the Dutch period; 1665–92, the period of English reorganization; 1692–1723, the years when town land was adequate and available; and 1724–75, the years of land shortage and imperial pressures. These changes were general and pervasive and in one direction, "from simpler forms to more complicated ones, and from fewer options to more choices" (pp. xii, 267–74). More specifically, in the course of the eighteenth century, townsmen experienced less local autonomy and more integra-

tion of the town into the county and provincial government, the declining role of town meetings and increased role of justices of the peace, and the proliferation of local offices. Kross's discussion of this process illuminates what the provincial government could do and what it could not do vis-a-vis the local institutions and problems. The economy of the town diversified under the pressure of land shortage; farmers took on secondary pursuits, such as artisanship, and preferred to sell their lands to their immediate family members and relatives. At the same time, the town's economic activities became more closely linked with New York City and even the transatlantic trade network so that townspeople owned and consumed more imported luxury goods, acquired more black slaves, and participated increasingly in money lending–mortgage business. Socially, the town became more stratified, the distance between the poor and the rich increased, but the standard of living went up for both groups and everyone had "access to more" (p. 270). The major victims of the land scarcity were the women to whom testators became less prone to bequeath land. In religion, the author notes a decrease in sectarian tendencies, more ecumenical and secular spirit, and a greater tendency toward separation of church and state.

The above short summary of the major lines of discussion in this book does not do justice to the comprehensiveness and detailed knowledge of its author. Despite the unavailability of some key records, such as militia and tax lists and censuses between 1715 and 1771, almost nothing of any importance relating to the history of Newtown escapes Kross's scrutiny. I can readily see why it took so long (about ten years) to complete her research. *The Evolution of an American Town* is an outstanding work that will serve as a fine guide both for community study methodology and for the questions researchers must ask and keep in mind.

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FRANCIS JENNINGS. *The Ambiguous Iroquois Empire: The Covenant Chain Confederation of Indian Tribes with English Colonies from its Beginnings to the Lancaster Treaty of 1744*. New York: W. W. Norton. 1984. Pp. xxv, 438. \$24.95.

This is the second volume of a projected trilogy on white-Indian relations in Colonial America. In the first, *The Invasion of America*, Francis Jennings detailed his revisionist history of New England; in the present volume, that of the middle colonies.

At issue in this most recent work is the character of the Iroquois "empire," which Jennings asserts was

a bit of "creative history" invented by the British, who donated it to "the Iroquois in order to claim it for themselves" (p. 11). Rather, what he discovers "hiding in thousands of source documents" (p. 368) is the Covenant Chain, a confederation between English colonies and Indian tribes. The picture Jennings draws of this confederacy is one in which most real power was wielded by white political elites who intrigued among themselves for political advantage. The best the Indians could do was to stave off for a time the inevitable outcome: white domination of their lands and lives.

Jennings's story is one replete with secret negotiations, mysteriously lost documents, and messages that do not mean what they apparently say. Gone are those ferocious, independent, wily Indians who held in their hands the fate of European control of the continent—those Indians so familiar to readers of Francis Parkman, the historian who has long been Jennings's favorite *bête noire*. Their place has been taken by docile creatures; albeit sometimes stubbornly intractable, they often dumbly followed white directives.

Not even the much-vaunted military successes of the Iroquois chronicled by earlier historians escape Jennings's label of "myth." "The great victories of the Iroquois," Jennings avers, "took place only in the West, against opponents lacking effective European support, and were confined to the brief span of 1649 to 1655" (p. 112). Otherwise, the Iroquois "achieved durable success only when backed up by European allies" (p. 43)—if in fact they achieved it at all. Jennings does not always see Iroquois victories where others have. For example, disregarding contemporary reports of the defeat of the Susquehannocks at the hands of the Iroquois in the mid-1670s, Jennings states that the Susquehannocks abandoned their Pennsylvania homelands because Maryland's governor, Charles Calvert, ordered them to do so at a "mysterious meeting . . . 'at Mattaponie' before the Indians came into Maryland" (p. 140). If Jennings had not overlooked the fact that Mattaponie was the name of Calvert's own residence, he would have realized the improbability of his conclusion: that Calvert went to his home not far from St. Mary's, Maryland's seat of government, and from there commanded the Susquehannocks, some hundred miles distant, to move to the Potomac River.

Jennings's enormously creative history could only be written in this century. Neither Parkman nor any of his nineteenth-century contemporaries could have produced such an interpretation; the Indian presence on the continent was then too formidable. But, for many Americans today, that presence is merely one of a few Indians living on distant reservations, often subject to the whim of decisions made in Washington. It is this simplistic view that Jennings has projected onto the past, discarding data that

does not conform to it and reshaping the remainder in accordance with his preconceptions.

ELISABETH TOOKER
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ISABEL THOMPSON KELSAY. *Joseph Brant, 1743–1807: Man of Two Worlds*. (Iroquois.) New York: Syracuse University Press. 1984. Pp. xii, 775. \$35.00.

Although well known in Canadian history, the Mohawk Joseph Brant has remained a generally obscure figure in United States history. Because of his role in the American Revolution and William L. Stone's ponderous nineteenth-century biography of him, he was more familiar to our ancestors than to our modern generation. Isabel Thompson Kelsay's new biography of this remarkable man will do much to rectify this neglect.

The book examines in detail Brant's extraordinarily varied career as student, interpreter, Indian Department official, Bible translator, war chief, Indian educator, statesman, and patriot. Although only a minor war chief at the beginning of the Revolution, Brant rose by his own abilities, attracted a following of both Indian warriors and white loyalists, and received a captain's commission in the British service. His energy and intelligence and his humanity in war commended him much to the British officers and often to his enemies as well. He traveled twice to Great Britain on behalf of his Indian people, was received at court by King George III, hobnobbed with the leading nobles and literati of the day, and had his portrait painted by George Romney and Gilbert Stuart, among others.

After the war, he led the loyalist Iroquois to Canada, where, once they were settled, he engaged in a struggle with British governmental officials to maintain Indian sovereignty and independence. One of his major concerns was forging an Iroquois and Western Indian confederacy to protect Indian rights and land from the inroads of American settlers and land speculators. His prominence in this endeavor brought him into contact with major American leaders such as George Washington, Henry Knox, George Clinton, and Aaron Burr. Ironically, in Canada he struggled agonizingly long with British officials for his people's right to alienate large tracts of their land to white speculators in order to gain ready money to support the emigre loyalist Iroquois, who found their land grant too large for their own farming and too small for hunting.

The Kelsay biography is enormously well researched, largely in primary sources, and is the product of thirty-years' effort. It tells us probably all that can ever be known about Brant and is written in a pleasing and skillful literary style. Particularly

valuable are the perceptive discussions of both the long controversy regarding the Grand River lands where the Iroquois settled after the Revolution, and the machinations of British officials in first promoting then undermining the western confederacy. These topics have not been so well covered before.

With all of its many merits, there are a few weaknesses in the book. Kelsay has a tendency to imagine what people were thinking or might have been thinking. She also brushes aside testimony of contemporaries, since some of it was recorded many years later, to deny that British officers and gentlemen "in the enlightened era of the American Revolution" (p. 303) paid for scalps. They merely paid Indians for war service. The untutored Indians imagined they were being paid for scalps. The fact is that the British paid Indians for both prisoners and scalps, since a scalp represented a prisoner. What embarrassed British officers was the proclivity of Indians to scalp live enemies as well as dead ones, and women and children.

Kelsay also does not understand the Delawares' status as "women" and speaks of "the hated petticoats" (p. 180). Actually, the Delawares' role was one of peacemakers, which was a woman's role and was not dishonorable.

Mrs. John Graves Simcoe's sketch of "H. M. Schooner *Onondaga*" on page 449 is a brig with the mainsail furled, and not a schooner.

These cavils aside, Kelsay has produced a fine and enduring biography of a major Indian leader.

BARBARA GRAYMONT
Nyack College

NELCYA DELANOË. *L'Entaille rouge: Terres indiennes et démocratie américaine, 1776-1980*. (Textes à l'Appui, Série Histoire Contemporaine.) Paris: François Maspero. 1982. Pp. 418. 95 F.

This book is an account of the formative years of a democracy, the United States of America, and of its historical and ideological links with native Americans. From 1776 until the present, native Americans have been dispossessed of their land, first by a colonial and subsequently by a capitalist system of government.

The book is divided into two distinct parts. The first, with the title "Des terres pour un État," shows how the apparatus of the state acted to acquire, administer, and bureaucratize territories and free people. In exposing the mechanics of dispossession with her analysis of bureaucratic and administrative practices in successive governments, Nelcy Delanoë fulfills the goals suggested by the subtitle of her work, *Terres indiennes et démocratie américaine*. The same subject, however, has been treated by many writers, several of whom—such as Francis Jennings,

Robert F. Berkhofer, Wilcomb E. Washburn, and Roy Harvey Pearce—have made important contributions.

One must ask why the writer bothers to press points that have nothing to do with her central thesis and that are already familiar to any student of native American history thanks to an abundance of scientific research. For instance, the broad subjects of prehistory and of demography are dealt with in a few pages, a dangerous exercise because it implies fatal shortcuts. To write that "buffalo, elk, camels, muskox and horses were doomed to extinction at the end of glaciation" (p. 20) is to be less than precise on the subject of different species of bison (*B. antiquus*, *B. occidentalis*, and the last *B. bison*) and to show her ignorance about the muskox. To add that the ground sloth "lived in herds in the company of wild horses and camels" (p. 20) suggests some fantastic bestiary. To infer, on the basis of Nuñez Cabeza de Vaca's narrative, that the Florida population was "meager" (p. 22) and that the Indians ignored the existence of each other is an invention: de Vaca never wrote in these terms about the whole of Florida.

Unfortunately, Delanoë has added a second part to her book in which the absence of preciseness gives way to confusion. In this connection, French editors should be advised to translate serious works rather settling, as they often do, for a potpourri. The second part of the book, "Le royaume et l'exil," consists of a chapter on the Algonquins and the Iroquois, followed by two others on the Delaware and the Cherokee, although there is no connection between them. Chapter 7 should have been placed at the end of the first part of the book.

The last chapter includes quotations from the *Walam Oum* of the Lenni-Lenapé, followed by a partial translation without the original pictograms. This might have passed muster if the author had devoted her whole book to the Delaware or to the theme of migration in North American Indian mythology. But, given her incomplete presentation of the Delaware Indians and the paucity of bibliographical references to them, her study has no interest because it has already been done. Why, for instance, did she neglect the work of C. A. Weslager or the Lewis Cass inquiries published by the former in *The Delaware Indian Westward Migration* (1978)?

It is regrettable that Delanoë did not end her book after she had written the first part. She made the serious mistake of publishing a doctoral dissertation without making the necessary corrections (which the doctoral jury must certainly have taken note of). Nor can Maspero, her editor, be excused from blame.

PIERRETTE DÉSY
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Montréal

V. V. SOGRIN. *Osnovateli SSHA: Istoricheskie portrety* [Founders of the U.S.A.: Historical Portraits]. Nauka: Moscow. 1983. Pp. 176. 65 k.

V. V. Sogrin's interpretation of the *Founders of the U.S.A.: Historical Portraits* marks a significant departure from the existing body of Soviet historiography on the American Revolution. Focusing on the leading personalities of the Revolutionary era and their political careers, Sogrin's primary concern is with political ideas, not class struggle. His work is the first to make extensive use of the published volumes resulting from the numerous editorial projects on the founding fathers that have come into being in the past few decades, and his immersion in those materials has given him a much greater appreciation for the ideological context in which America's Revolutionary leaders carried out their activities. Moreover, Sogrin has been generally successful in incorporating some recent American scholarship into his own work, including the work of Alfred Young and Pauline Maier, although he does not seem to have consulted the work of Bernard Bailyn. The book is also well written, in a lively style free from the cliché-ridden prose often characteristic of Soviet histories. It is obviously intended for a large audience, for one hundred thousand copies have been printed, an astonishing large initial printing for a book on American history.

Sogrin selects a handful of the founding fathers—George Washington, Samuel Adams, Alexander Hamilton, Thomas Jefferson, and James Madison—as the subject of the five main chapters of his book. Around these men he skillfully weaves the main political events of the period from 1763 to 1815, introducing briefer sketches of the lives and ideas of Benjamin Franklin, Thomas Paine, John Adams, and several others along the way. His main thesis is that the historical achievement of the founding fathers was to lead a progressive, anticolonial movement of national liberation embodying democratic ideals that represented a sharp break with the feudal and authoritarian institutions of the English monarchy. It was, then, in his view a genuine political revolution. His interpretation of the course of that revolution often contains echoes of some of the themes of Progressive historiography; he argues, for example, that complete democratization of American life was prevented by the uneasy alliance between southern planters and the northern bourgeoisie to defend the interests of their class against the aspirations of more popular, lower-class elements. And, not surprisingly, his interpretations of these developments encounter some of the same difficulties as those of many of the Progressive historians. Sogrin does not, for example, always make clear precisely what he means by "class" in the

American context, especially when he describes both southern planters and northern merchants alternatively as distinct and antagonistic classes and as part of the same class.

But Sogrin's treatment of the evolving political thought of the founding fathers shows considerable sophistication. In dealing with Jefferson's and Madison's oscillation between egalitarian and elitist modes of thought, for example, Sogrin resists the temptation to depict those two as representatives of a hypocritical, reactionary planter class (as has been frequently the case in Soviet historical writing), and instead analyzes their actions within the confines of the political and social realities of their time. In this respect his analysis of Jefferson's attitude toward slavery is especially revealing. According to Sogrin, slavery was morally repugnant to Jefferson, judging by his private correspondence. Yet in public Jefferson was reluctant to press his case for fear of alienating powerful leaders in Virginia whose support was vital in the struggle for national unity. Similarly, his discussion of the formation of the Republican party reveals his awareness of the complex social structure standing behind the differences between the Virginians at one end of the party's political spectrum and the Pennsylvanians at the other.

Inevitably, in such a highly compressed treatment there are omissions, oversimplifications, and occasional lapses of judgment. There is also a highly polemical final chapter in which current American policy is characterized as a betrayal of the fundamental principles of the founding fathers. Overall, however, the book will give a more sympathetic, balanced, and comprehensive portrayal of the origins of the United States and the principles of its constitutional system than anything else written by a Soviet historian.

RICHARD BEEMAN

ALFRED J. RIEBER

University of Pennsylvania

JOYCE APPLEBY. *Capitalism and a New Social Order: The Republican Vision of the 1790s*. (Anson G. Phelps Lectureship on Early American History.) New York: New York University Press; distributed by Columbia University Press, New York. 1984. Pp. x, 110. \$16.50.

Originally presented as the Phelps Lectures at New York University, the four essays in this well-focused volume are thoughtfully interpretive and full of substance. So skillfully has Joyce Appleby distilled her research and related it to the scholarship of the past twenty-five years that any summation of her arguments can only suggest the main strands.

The author sees the differences between the Federalists and the Jeffersonian Republicans as fundamental: the Federalists defending an older order, the Republicans sharing a vision of a new society. The essays draw on a rich scholarship about colonial American society that has shown how deeply conservative many communities were and how much was at stake in the challenges to accepted thinking after the Revolution. This scholarship makes it possible to accept the Federalist presumption in the 1790s that they were speaking for a conception of civil order rooted in American values. Appleby suggests that the test for distinguishing Federalists from Republicans is whether they expected the future to be a continuation of the known or to be fundamentally different. Republicans envisioned a classless social order and expanding prosperity based on the Atlantic economy that provided markets for the produce of American farms. The author rejects the interpretation that the Jeffersonians represented an American version of the English country party with Republican and Federalist conflicts replaying the wrangles between court and country.

Appleby stresses ideas. She sees Republicans coalescing around a "common vision about the reform of politics and the liberation of the human spirit" (p. 4). She emphasizes ideas in the Republican triumph of 1800. "They did it with words," she writes (p. 78), uniting voters by their vision of classlessness and their faith in a better future. To Appleby the election of 1800 was as revolutionary as Jefferson conceived it to be, and it decisively changed American political culture. It was the last time that any group of Americans seriously seeking power in the national government championed hierarchical values or deferential political practices; it brought the end to aristocratic values in American politics.

The author's analysis is rich in tracing the roots of ideas and the changing meaning of words and concepts. She gives particular attention to the background of English commercial growth in the seventeenth and eighteenth centuries that brought the American colonies into the new commercial capitalism of the Atlantic trade. This is shown to be more relevant to understanding the 1790s than the later development of industrial capitalism, which has tended to shape the perspectives of many historians. Appleby believes that the concept of capitalism as an independent force bending men and nations has obscured the role that the expectation of commercial growth played in the social thought of the Jeffersonians, and she sees the perspective of profit-grabbing industrial capitalism as responsible for some historians labeling the Jeffersonians as anti-capitalistic. In the eighteenth century, commercial farming was a progressive economic force making possible a better future, and it provided the capital-

ist underpinning for the popular political movement of the Jeffersonians. "Where Republicans differed from Federalists," Appleby writes, "was in the moral character they gave to economic development" (p. 49). The promise of future prosperity encouraged them to visualize a future far different from the past. In successfully challenging the Federalists, the Republicans "endowed American capitalism with the moral force of their vision of a social order of free and independent men" (p. 104).

This thoughtful and well-written book, grounded on wide learning, deserves a large audience.

NOBLE E. CUNNINGHAM, JR.
University of Missouri

PHILIP SCRANTON. *Proprietary Capitalism: The Textile Manufacture at Philadelphia, 1800-1885*. New York: Cambridge University Press. 1983. Pp. xiii, 431. \$34.50.

Relatively small, unincorporated firms, partnerships, and individually owned enterprises made Philadelphia the foremost manufacturing center of textiles in the United States by 1880. Philip Scranton argues that such firms, from the beginning of the century onward, were typical of the industry and deserve, therefore, the attention of historians who in the past have been inordinately preoccupied with the classic Lowell-style corporations. The imbalance is correctable, but not without difficulty. Small firms leave scanty records. Yet Scranton has managed to pack his book with figures, tables, and biographical details (sometimes excessive and irrelevant), with sociological, economic, and technological observations, and with scattered comments on the deficiencies of capitalist morality.

One transcendent characteristic of the industry, including all kinds of specialized and unspecialized firms from spinning and dyeing to carpet and cloth manufacture, was its ephemeral nature—the rapid appearance and disappearance of firms over the years. Another was the skill of Philadelphia's work force and its industry's managers. Unlike the machine tenders and corporate managers of Lowell, to whom Scranton repeatedly refers, the workers and proprietors of Philadelphia's textile firms were remarkably versatile; they used different raw materials and developed different product lines as market forces dictated. This flexibility was evident particularly during the Civil War when the cotton supply dried up; the industry, nevertheless, gorged itself on government contracts, producing woolen uniforms, blankets, and shoddy in profusion. Lowell, by contrast, failed to meet the challenge.

Native-born genius deserves little credit for the achievement. Irish and English immigrants risked their capital as owners, or occasionally their jobs as

strikers. They constituted a reservoir of expertise that poured into the predominantly American-born community, maintaining largely on their own one of the city's major industries.

These conclusions indicate that Scranton probed the record with determination and persistence in his "systematic analysis" of the "matrix of accumulation" that he represents as characteristic of proprietary capitalism. Precisely what identifies a "systematic analysis" proclaimed so frequently in recent literature, is anybody's guess. The "matrix" is more accurately defined. It encompasses a range of factors of production and distribution: raw materials, marketing, technology, and finance among them. Some are considered at length; others slip into obscurity presumably because the information disappeared with the firms. For example, Scranton tells us little about credit or about capital at the entry stage; yet these are significant considerations with admittedly vulnerable, small firms. Owners, Scranton observes, frequently "withdrew" from business with a "competence." How many went bankrupt, what the matrix of loss was, is unknown.

Proprietary Capitalism does not meet the standards established by Anthony F. C. Wallace's *Rockdale*, a study of textile manufacturing in a rural setting close to Philadelphia, which is distinguished for its sociological and technological scope and literary polish. Scranton's work, however, has undeniable merit. It copes with the seeming paradox of unstable firms that rose sporadically and disappeared suddenly yet were responsible for the emergence of Philadelphia as an established center of the textile industry based on skills as much as, or perhaps more than, on available capital resources.

NATHAN MILLER
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JAMES H. CASSEDY. *American Medicine and Statistical Thinking, 1800-1860*. Cambridge: Harvard University Press. 1984. Pp. x, 306. \$22.50.

Hand in hand with antebellum expansionism, according to James H. Cassedy, came Baconian-inspired data collection and analysis, especially applied to improving the new nation's health. "The most intelligent and articulate" physicians (p. viii) showed interest in number gathering, and they, in alliance with other reformers, used statistical analysis, unsophisticated by modern standards though it was, to try to improve the unsanitary conditions of growing cities, the care of the mentally ill, the image of the medical profession, and public health. Their efforts steadily improved, by Cassedy's analysis, until reaching their nineteenth-century peak in 1860.

This book describes the efforts of the early medical statisticians and shows how their interests in upgrading their institutions and profession led them to numerical calculations. It also posits that statistics improved medicine's status and effectiveness, especially in obstetrics and surgery, because the amassing of data led to new insights and an understanding of health problems. Cassedy suggests a close correlation between medicine's professional claims and the use of statistics. His descriptions of these trends are very well done, and he adds substantially to our understanding of medicine in this period. Unfortunately, however, he spends too little time analyzing the processes by which the burgeoning methodology made its impact, and the book, which is an important introduction to this field, ultimately fails to satisfy our heightened curiosity about the relationship between antebellum statistics and medical theory and practice.

Take, for example, the use of statistics to prove or disprove the value of homeopathy, one of the many alternative systems of medical practice that flourished in the period. One of the best sections of the book is Cassedy's description of the statistical applications of homeopathic practitioners, who prescribed infinitesimal doses of medicine to cure disease. Homeopaths used the results of their careful record keeping to try to win patients; regular physicians tried to show why the homeopathic statistics were misleading and to present their own counter statistics. Many people sought homeopathic aid in antebellum America. But was it because they were more convinced by homeopathic statistics? What was the popular impact of the new methodology? Cassedy stops short of confronting this question, and instead he calls our attention to "a generation that thrilled to the marvels of a P. T. Barnum" and "the ordinary person's infinite capacity for medical variety" (p. 134) to explain homeopathy's attraction. This begs the very important question that Cassedy's book raises and leaves readers still needing an analysis of the statistics themselves and an examination of the lure and importance of statistics in general. Did they represent the "new" science within and outside of the medical community? How did number gathering in this example, or in many others presented throughout the book, matter?

Cassedy continues here his description of medical statistical thinking in the United States that he began in *Demography in Early America* and that he will continue in future volumes. In both books to date he presents a thorough view of the literature and very interesting data in a well-organized and useful manner, and the subject is one of growing interest to historians. Both books are recommended reading.

JUDITH WALZER LEAVITT
University of Wisconsin

MICHAEL B. KATZ. *Poverty and Policy in American History*. (Studies in Social Discontinuity.) New York: Academic. 1983. Pp. xii, 289. \$27.50.

Michael B. Katz is one of our most prolific and challenging social historians. Having written first about American education and then about social structure and capitalism, he has now turned his attention to the history of American welfare. *Poverty and Policy in American History* is largely a collection of essays on individual topics rather than a systematic analysis of welfare. As a social historian familiar with the use of local data and quantification, Katz offers a series of case studies. The first details the experiences of a single family with the Philadelphia welfare system in the early twentieth century; the second is a detailed demographic history of the Erie County (New York) poorhouse in the nineteenth century; the third is a discussion of late nineteenth-century social science data developed by New York State officials that was used to stigmatize paupers; and the final section is a historiographical discussion of the works of Paul Boyer, David Rothman, and Roy Lubove.

Katz writes within a quasi-Marxian framework, and his book is clearly addressed to contemporary concerns. Welfare in America, he insists, has traditionally rested on the presumption that poverty was a moral rather than an economic condition; individuals were poor because they were weak, lazy, and indolent. Poverty, therefore, was an individual rather than a social condition. Indeed, Americans rarely distinguished between poverty, crime, and ignorance; these categories were for the most part identical.

Such perceptions, writes Katz, are historically erroneous, and the purpose of his book is to shed light on the nature and sources of dependency. In his eyes dependency was in fact a function of the organization of economic and political life in America rather than a consequence of moral and character deficiencies. Recipients of welfare, whether provided with indoor or outdoor relief, were overwhelmingly casualties of unemployment and economic depression. Many paupers were older working-class persons lacking families to care for them, and others were temporarily out of work. The tramp population was composed of males traveling in search of work. For most working-class people the welfare system provided temporary assistance; it was not a way of life. The most valuable part of Katz's book is his demographic analysis of a nineteenth-century poorhouse, which demonstrates that the unemployed used it for brief periods of time, whereas the aged and chronically infirm often remained until they died.

Katz's work has all of the virtues as well as some of the vices of social history generally. His case study

approach is highly illuminating and reinforces the view that the working poor were not lazy, immoral, or quiescent. The discussion of national developments is less successful, partly because it is not possible to leap from a few local case studies to a discussion of two centuries of welfare that ignores regional variations and changes over time and partly because his analytic framework is too ideologically confining. My disagreements with Katz notwithstanding, this is a significant work because it focuses on the right problems, provides needed examples of the kind of research that ought to be pursued, and asks stimulating and challenging questions.

GERALD N. GROB
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EDWARD L. AYERS. *Vengeance and Justice: Crime and Punishment in the 19th-Century American South*. New York: Oxford University Press. 1984. Pp. ix, 353. \$24.95.

In *Vengeance and Justice*, Edward L. Ayers has written a valuable book. Focusing on the criminal justice system in the nineteenth-century American South, Ayers has made an ambitious effort to place southern responses to crime in the broadest possible social and cultural context. The result is a pioneering and stimulating examination of a neglected aspect of southern history and a significant addition to the historiography of crime and punishment in America.

Ayers divides his book into two sections, the first focusing on antebellum criminal justice and the second on crime and punishment in the South from the opening of the Civil War to about 1900. His strategy throughout the book is to look intensively at criminal justice practices in three Georgia settings—a rural, upcountry county, a plantation-dominated county, and urban Savannah—generalizing from his findings by referring to sources from elsewhere in the South. The method is effective, giving depth as well as breadth to his conclusions.

Ayers begins his study by arguing that the development of southern criminal justice involved a combination of traditional attitudes—including the concept of honor—with the social and economic demands of slavery. He shows how this combination of factors influenced the introduction of the penitentiary into the South during the first half of the nineteenth century. He shows, too, how debates over the establishment of prisons, as well as problems associated with the day-to-day administration of justice, brought into focus strong tensions underlying white society and culture in the Old South.

The second half of the book continues to stress the interaction of cultural with social and economic factors as it traces the adjustments of southern

criminal justice to the sudden creation, after emancipation, of a large free black population. As Ayers convincingly demonstrates, justice was never color-blind in the postbellum South, and, if the conclusion is no surprise, the evidence he uses to support it is nonetheless impressive. His accounts of the notorious convict lease system and of lynching reinforce the picture of a region where justice was a minor concern, at best, in the prosecution and punishment of black people accused of criminal acts.

Recent social historians have produced several excellent monographs on the history of criminal justice in America. Virtually all of these studies, however, have dealt mainly with the North and have related American developments to the social and cultural context provided by northern industrialization and urbanization, as well as to similar developments taking place across the Atlantic, particularly in England. Ayers demonstrates that most of the issues preoccupying northerners were familiar to southerners, too. Nevertheless, as Ayers also shows, southern approaches to handling crime and punishment had a distinctive regional twist throughout the nineteenth century. By indicating the forces producing this distinctiveness, Ayers deepens our understanding not only of southern society, but also of the factors that have influenced the evolution of the American criminal justice system.

DICKSON D. BRUCE, JR.
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SUZANNE LEBSOCK. *The Free Women of Petersburg: Status and Culture in a Southern Town, 1784-1860*. New York: W. W. Norton. 1984. Pp. xx, 326. \$24.95.

In her tight, excellently researched, microstudy of Petersburg, Virginia, Suzanne Lebsack not only sets a model for doing women's history based on local records but also tests and challenges several theoretical positions in the field. The result is an elegant and methodologically sophisticated book of major importance.

Lebsack tests Mary Beard's thesis that equity law in regard to married women's property had guaranteed British and American women a substantial degree of freedom. She finds that Beard was correct in pointing out the importance of studying documents pertaining to estates and considers them an "unusually sensitive index to the changing status of women of the propertied classes" (p. 54). She finds Beard correct in that in Petersburg during the antebellum period the use of separate estates increased markedly, which gave women more autonomy. But Lebsack notes that the increased use of property conveyances did not give women unre-

stricted control of their property. She also finds an increased tendency by propertied men to give women executor's roles in their wills, increased use of premarital contracts, more liberal grounds for divorce and separation, and an increasing participation of women in the management and acquisition of property. From her study of wills, Lebsack concludes that there was a dramatic rise in the number of women who wrote their wills in the period she studied, and she finds as well an increased participation by women in real estate and credit transactions.

This leads her to conclude that the thesis of the decline of women's status in the antebellum period needs revision, but she is critical of those who see this period as one of progress for women. Specifically, she challenges the thesis that women's progress can best be measured by their status in the family and their gains in domestic power, and she denies that whatever gains were made were owed to the rise of "companionate marriages" and to love and affection among couples. Changes in married women's legal and economic status were won "not because men changed their minds or even their hearts, but as a result of an attempt to escape the terrible uncertainty of the nineteenth century economy" (p. 53).

Men settled estates on their wives to escape bankruptcy and creditors' claims; men made women executrixes of their wills when the law made clear that the widow would lose such rights in case of remarriage. In a most interesting chapter on women's organizations, she shows that women had a virtual monopoly on organizing charitable work and founding charitable institutions until the late 1850s, when men took over institutions women had run successfully for some time and relegated women to auxiliary roles. At the same time, she notices various signs of women's public deference to men, which had not occurred at the beginning of the century. Women, for example, who earlier had signed documents with their own first name or initial now signed themselves "Mrs. John Smith." The strictures against women speaking in public also increased. An example was the annual ceremony when women presented a flag they had made to the militia company. Although in 1825 and 1831 women made public speeches at this ceremony, men spoke after 1847 in behalf of "weak and defenseless" women. Lebsack argues that this was an indication of male anxiety over women's increasing autonomy and an effort to set boundaries to women's choices.

Lebsack offers very persuasive evidence of the existence of a women's culture. Women wrote their wills differently from men: men tended to distribute their property even-handedly, by formula, while women individualized their gifts, made careful explanations, and favored daughters. Women were

kinder to slaves in their wills than men and more likely to set them free. Lebsock similarly analyzes women's property transactions and their organizational activities and compares them to the activities of men. This allows her to give a rich texture to her narrative and offers the reader an unusual perspective on the social history of a community.

Lastly, Lebsock is particularly attentive to differences in regard to class and race. Her narrative is filled with fascinating insights on the lives of free black women, most of whom lived in abject poverty. Yet in 1860 black women were half of all black property holders, as against 25 percent of white women property holders. This was because a large proportion of black women remained single and were heads of households. Lebsock manages to extract rich, humanistic detail from municipal and county records and to give us a picture of women, both black and white, as active agents of their own destiny.

According to Lebsock, Petersburg women acquired between 1874 and 1860 a greater degree of autonomy and power in the private sphere, but confronted new forms of subordination in the public sphere. In all areas the gains women made gains lagged behind those of men in their group. Given these conclusions, it appears that Lebsock has offered supporting evidence for both those who see the antebellum period as one of decline and those who see it as one of advance for women. It simply depends on what aspect of women's status one looks at and whether one looks at women's status in isolation or in comparison with the status of men of their race and class. Lebsock's conclusion that "positive change in the status of women can occur when no organized feminism is present" (p. 240) and her suggestion that "for the majority of women, North and South may not have been so very different after all" (pp. 243-44) until after the Civil War are both challenging and deserving of further exploration in comparative studies. All such studies will have to be measured against Lebsock's fine book.

GERDA LERNER
University of Wisconsin

MARY KELLEY. *Private Woman, Public Stage: Literary Domesticity in Nineteenth-Century America*. New York: Oxford University Press. 1984. Pp. xx, 409. \$24.95.

The most popular nineteenth-century American writers were women—"a damned mob of scribbling women," said Nathaniel Hawthorne—who wrote fiction about, and for, women. Interpretations differ as to how strongly and how consciously they affirmed or struggled against the gender constraints imposed by their culture, but Nina Baym's *Women's Fiction* (1978) and Mary P. Ryan's *The Empire of the*

Mother (1982) have shown that women writers merit attention both as best-selling fabulists and as unusually articulate "working women."

Mary Kelley's *Private Woman, Public Stage* is the most ambitious study of these writers to date. On one level it offers a group career profile of a dozen of the most successful: Catharine Maria Sedgwick, Caroline Howard Gilman, Caroline Lee Hentz, Maria McIntosh, Harriet Beecher Stowe, Sara Par-ton (Fanny Fern), E. D. E. N. Southworth, Susan Warner (Elizabeth Wetherell), Mary Jane Holmes, Maria Cummins, Mary Virginia Terhune (Marion Harland), and Augusta Evans Wilson. But Kelley is less concerned with the external features of their working lives than with the conflicted sense of self that she finds in every one of the "literary domestics" (p. viii). Daughters of elite families, they were relatively well educated. What "social status gave," however, "gender took away" (p. 112): they were expected and conditioned to occupy only the domestic sphere as wives and mothers. The coexistence of this all-absorbing domestic identity with the impulse to step into the male domain of literature, says Kelley, made for a permanently "divided self" (p. 127). Writing could be legitimated only on the ground that it served the domestic value of economic survival for the family.

This conflict, Kelley argues, carried over into their fiction, which told of "the woman's promise and betrayal in the home" (p. 335). Although "compromised and crippled as writers" (p. 335) by their necessary allegiance to domesticity, they put that situation on record, and for Kelley that is their major achievement. Especially effective in this regard is the treatment of works whose heroines give up literary success for marriage. Kelley successfully challenges any lingering notion that these writers were uncomplicated conformists.

Lavish with illustrations from both the fiction and the private papers in which she has done pioneering research, Kelley offers a richly detailed account plentiful with new findings. Yet if the description is thick it is also rather claustrophobic. The literary domestics, especially the earlier ones, are expertly placed in social history, but their relationship to literary culture could be more fully specified and better informed. Authorial anxiety and self-depreciation seem to be attributed to gender alone. And, although Stowe and Sedgwick successfully resist reduction to the composite consciousness of the literary domestics, the figures in the group portrait leave an impression of general despondency that Kelley may not have intended.

Kelley's emphasis on the web of domesticity may qualify Baym's argument that these writers advanced a straightforward if moderate feminism, but Baym nevertheless provides a clearer and more sophisticated guide to the novels in question. For the

inner lives of their creators, however, readers will want to turn to *Private Woman, Public Stage*, an important book that deserves a wide audience.

HOWARD KERR
University of Illinois,
Chicago

JED DANNENBAUM. *Drink and Disorder: Temperance Reform in Cincinnati from the Washingtonian Revival to the WCTU*. Urbana: University of Illinois Press. 1984. Pp. xii, 245. \$22.50.

In the antidrink crusade in Cincinnati and in Ohio from the 1840s through the 1870s, Jed Dannenbaum finds "one of the most vital and revealing issues of recent history" (p. 12). His work carefully and convincingly affirms the revisionist thesis that the temperance and prohibition impulse was a significant part of the liberal-reformist tradition and that it represents some of the most attractive—rather than the least attractive—features of American life. The thesis is widely accepted, so the question here is, what can we learn from studying temperance and prohibition in Ohio? The answer is, we can learn much that is important and interesting—about the impact of German immigration, about the character of urban development and the complexities of concurrent political alignments, about the Civil War years, and about the changing role of women during years of considerable turmoil and antagonism.

Dannenbaum identifies three phases of antidrink energy, each a distinct social effort to achieve a measure of control over a disorderly society: first, the fraternal movement of the 1840s, which in providing fellowship, moral codes, protection, and rituals assisted the Ohio middle class in its uncertain transition from rural to urban living; second, the period of Maine-Law political dynamics of the 1850s, which demonstrated how the Whig party, well before the Kansas-Nebraska Act, was torn apart by nativism and prohibition; and third, the sustained direct action of women, which, when prohibition lost its political potential later in the 1850s, transformed the movement into violent waves of vigilantism.

The analysis of Whig and Know-Nothing politics is revealing—the sort of first-rate regional history that demands the attention of whoever now wants to think clearly about Whigs and Know-Nothingism. But Dannenbaum's most intriguing contribution is his showing how a "massive vandalism" (p. 205) spread through hundreds of towns and cities in the 1850s when women realized that they should never have supposed their grievances against saloons could have had any political resolution. (Claiming the Boston Tea Party as "a moral precedent"

[p. 198], one band of women vigilantes were defended in an Illinois court by Abraham Lincoln.) Although soon quieted by the Civil War, the passion reignited in the 1870s. Thus, the "Women's Crusade" of 1873, which is seen as a beginning in many histories, was not a singular or spontaneous episode but rather a climax of a major social protest.

As good as the book is, one wishes that Dannenbaum had done more with a narrative-analytical account of the twenty-year crusade and with what it says about gender roles and the American bourgeois family. But to wish this is not to diminish the book he has written. He makes thoughtful use of the abundant recent scholarship and of the rich primary sources in Ohio. Except for some unpredictable metaphors ("instances of praying crusades suddenly began to snowball" [p. 212]) his work is stylistically impressive. The book is also handsomely printed, bound, and illustrated.

NORMAN CLARK
Everett Community College

WILLIAM L. VAN DEBURG. *Slavery and Race in American Popular Culture*. Madison: University of Wisconsin Press. 1984. Pp. xiii, 263. Cloth \$29.50, paper \$12.50.

William L. Van Deburg had a good idea for a book but, unfortunately, failed in his execution of it. *Slavery and Race in American Popular Culture*, as a result, is a badly flawed book that is mistitled and reflects the author's mixed purpose. Van Deburg's original intent was to write a historiographical study of slavery, but he then decided also to consider literature and drama to make the book into one on popular culture. He did not, however, abandon his first intent; he merely added to it. Using "period novels, poetry, short fiction, history texts, play-scripts, songsheets, and feature films," he hoped to find "the cultural underpinnings of American opinion on matters of slavery and race" (p. xi).

At the onset Van Deburg failed to make clear exactly how he proceeded in his analysis. Two shortcomings are most obvious. He does not indicate what he means by popular culture or what standards he used for including the works he analyzes. Why *Time on the Cross* and not *The Problem of Slavery in Western Culture* or *The Black Family in Slavery and Freedom* or *Been in the Storm So Long*? An even more serious failure is the lack of clarity about whether he is discussing race and slavery as separate entities, which is presumed in the title, or race only as connected with slavery, which seems to be his practice. The resulting confusion leaves the reader with little sense of accomplishment at the end of the book.

Part of the problem is that the book is a very short one, shorter even than the total of 263 pages indicates. Only 160 of these pages are text; the remaining 103 are footnotes, index, and bibliography. Within the limited span of text Van Deburg attempts to survey the years from 1619 to 1980, an impossible task. Moreover, despite 70 pages of footnotes and a large bibliography, significant works in each area covered are missing. The result is a book where the Harlem Renaissance is slighted and *Cane* appears only in a footnote.

To compound the difficulties, the chronological organization of the book, although specified in chapter titles, is frequently violated. A section on black history beginning with pre-Carter Woodson days appears in a chapter on developments from 1965 to 1980 after an earlier chapter that analyzed the work of John Hope Franklin.

Finally, Van Deburg sometimes displays a tendency to be less than frank. In his discussion of *Roots*, he says that "A predominantly white group of critics challenged the black author's claims to accuracy" (p. 145), mentioning only in a footnote buried at the end that Margaret Walker, a black author whom he had just praised, sued Haley for plagiarism.

Contrary to dust-jacket claims that this is "the first interdisciplinary study of American popular culture and its historical attitudes toward slavery and race," the book is instead a mélange of the historiography of slavery, of the image of the slave in plantation novels, and of blacks' counter-images, none done in much depth.

DWIGHT W. HOOVER
Ball State University

JOHN R. MCKIVIGAN. *The War against Proslavery Religion: Abolitionism and the Northern Churches, 1830-1865*. Ithaca: Cornell University Press. 1984. Pp. 327. \$29.95.

The sectional schisms in the Methodist, Baptist, and New School Presbyterian denominations growing out of the slavery controversy are well known to historians. Less familiar is the impact of abolitionism on the northern churches themselves and the formation of splinter groups within them. This fine monograph has two central themes developed in tandem, more or less chronologically. One is the effort of abolitionists to convert clergymen and church people of the North to their cause. The other is the quarrel among the leaders of these churches over endorsement of abolitionist goals such as the denunciation of slaveholding as a sin against God and the cessation of Christian fellowship with congregations including slaveholders.

After a brief introduction and a short account of church resolutions against slaveholding adopted in the Revolutionary era, the author proceeds to an analysis of the interaction between the churches and the American Anti-Slavery Society (AASS) during the period from 1833 to 1840. This organization attracted a small but vocal minority in the northern religious community. John R. McKivigan sees religious issues as an important factor in the abolitionist split of 1840 that resulted in the formation of the American and Foreign Anti-Slavery Society, whose leaders were more church-oriented than the Garrisonian remnant of the AASS. He presents tables and an appendix showing the denominational affiliations of officers of the old and the new organizations.

After due attention to the secession of southern Methodists and Baptists in 1844 and 1845, he moves on to the "comeouter sects" that withdrew from northern branches of these and other denominations: the Wesleyan Methodist Connection, the American Baptist Free Mission Society, and the Free Presbyterian Church, among others. The controversy among Friends and the important role of the Hicksite Quakers in the antislavery movement are somewhat neglected. So, also, is the work of female abolitionists such as the Grimké sisters and Lucretia Mott.

The author discusses the impact of abolitionism on the "Benevolent Empire" and traces the history of new interdenominational groups such as the American Missionary Association and the Church Anti-Slavery Society, which were formed as a result of dissatisfaction with the stand of the older organizations on issues involving slavery. McKivigan then deals with the relationship of the churches to the political abolitionism of the 1840s and 1850s. He divides antislavery advocates into three categories: Garrisonians (who tended to be anticlerical), political abolitionists, and "Christian abolitionists." He finds that abolitionism made its greatest progress in evangelical denominations such as Methodists, Baptists, and New School Presbyterians and enjoyed its least success among conservative denominations such as Roman Catholics, Episcopalians, and Old School Presbyterians. His final chapter deals with the ultimate conversion of northern church people to emancipation in the wake of the Civil War.

The book originated as a doctoral dissertation. It is based on diligent research in primary sources and reflects recent scholarship. It is well written and, except for a large number of qualifying statements introduced with "however" or "nevertheless," is easy to read.

IRA V. BROWN
Pennsylvania State University

PHILIP S. FONER and JOSEPHINE F. PACHECO. *Three Who Dared: Prudence Crandall, Margaret Douglass, Myrtilla Miner—Champions of Antebellum Black Education*. (Contributions in Women's Studies, number 47.) Westport, Conn.: Greenwood. 1984. Pp. xviii, 234. \$29.95.

The story of the courageous attempts of Quaker Prudence Crandall to open her school to the training of black females, and the vicious attacks she received from her neighbors in Canterbury and throughout Connecticut, has been recounted many times in general surveys of the educational conditions for free blacks in the antebellum North. Initially, Crandall was shocked and dismayed by the town's response, but she decided to fight the legal and extralegal maneuvers to close the school, and she received a great deal of support from William Lloyd Garrison, Lewis Tappan, and other "radical abolitionists." Less has been known about other attempts by white women to provide schooling for free blacks in antebellum America, but in *Three Who Dared* we now have a well-written and well-documented account of the trials and tribulations of Prudence Crandall by Philip S. Foner and extensive examinations of the lives and educational exploits of Margaret Douglass and Myrtilla Miner by Josephine F. Pacheco.

What was even more daring about the educational activities of Douglass and Miner was that they attempted to start schools for free blacks in southern cities. Margaret Douglass was born in the District of Columbia and raised in Charleston, South Carolina, as a "Christian gentlewoman" of very limited means. In order to support herself and her daughter, Rosa, once they left Charleston and settled in Norfolk, Virginia, "Douglass became a seamstress and specialized in making vests, those elegant adornments of nineteenth-century men" (p. 57). To get a little extra money, she decided to open her home to instruction of free blacks who were eager to learn but only had available a Sunday school class taught by the "wives and daughters of the leading citizens of Norfolk" (p. 58). It was, of course, against the laws of Virginia to teach free blacks to read and write, but Douglass figured it was all right since the members of Christ Church were instructing free black children. She was wrong, however, and, once city officials were alerted, Douglass was charged, tried, defended (by herself), convicted, and sentenced to one month in jail in January 1854. Unlike Prudence Crandall, Margaret Douglass harbored no abolitionist sentiments. She had started her school as part of her "Christian duty" to provide the "Light of Christianity" and "saving knowledge of the Gospel" to free black children. Norfolk public officials evidently viewed the two perspectives as interchangeable.

Myrtilla Miner was born and raised in North Brookfield, New York, and received her schooling at the Young Ladies Domestic Seminary in Clinton, New York. Following unsatisfactory teaching stints in Rochester, New York, and Providence, Rhode Island, in 1844 and 1845, Miner took a position at the Newton Female Institute in Whitesville, Mississippi. This was her first exposure to the South and American slavery, which filled her with "horror and despair." When she left after less than three years and returned home, at some point (we have no idea exactly when) she got it into her head to start a school in Washington, D.C., for the instruction of black females as teachers. With less than one hundred dollars, Miner went to Washington in 1851 and eventually was able to find a suitable room for her school. Through the financial support of Philadelphia Quakers and other philanthropists, Miner's school remained open until the Civil War, despite the legal and physical attacks on her and her "spiritually-inspired" enterprise.

Foner's sketch of Prudence Crandall is embedded in a comprehensive survey of the abolitionist campaign and activities of the era. This is not entirely the case in the essays on Douglass and Miner by Pacheco. These are well-documented biographical portraits that present only limited information relevant to understanding the overall thrust of antebellum educational activities among free blacks. The significance of *Three Who Dared*, however, lies in its thorough presentation of the stories of three white women who suffered and struggled for Afro-American advancement in antebellum America.

V. P. FRANKLIN
Yale University

ROBERT V. REMINI. *Andrew Jackson and the Course of American Democracy, 1833-1845*. Volume 3. New York: Harper and Row or Fitzhenry and Whiteside, Toronto. 1984. Pp. xxiii, 638. \$27.95.

This long and richly detailed volume completes both Andrew Jackson's rarely serene life and Robert V. Remini's forceful attempt at a historiographic counterrevolution. Eschewing the contemporary turn away from the great man centered view of politics, he has revived, to the fullest, the leisurely, instructive, sympathetic, life and times grand biographical tradition. Well aware of recent scholarship and respectful of certain of its elements, Remini makes it clear that to him the emphasis on structure, trends, the inarticulate and voting masses, and their culture and behavior is useful to illuminate but never to dominate understanding. Thus, as in earlier volumes, he rarely strays from Jackson's presence, both in the public arena and in the trials of an anxious, often difficult, private life. The author covers a

great deal, from French spoliations to banking, Indian removal, and the vicissitudes of politics, personal debt, and persistent ill health. The organization is chronological, the presentation full, if wordy and sometimes repetitious. Discussions of broader trends, when present, fall between succinct and perfunctory. There is little psychological speculation about Jackson's motives or those of his followers and supporters.

Much of Remini's Jackson is familiar: passionate and single-minded, he was committed to friends, his party, a powerful presidency, and a dominant union. Old Hickory's motives were of the best. In comparison, most others, friends and foes alike, pale into incompetence, shallowness, or sordidness. Jackson's policies were beneficial and successful, his foreign policy was strong but cautious, and his domestic goals were on the right track. As the United States moved from republican fears for a fragile nation to the exuberant optimism of egalitarian nationalism, a dynamic and commanding Jackson was at the center of it all: its leader, its impelling force, and, soon enough, its symbol.

Nor were Jackson's democratic assertions claptrap. The old eighteenth-century idea of balanced government was not for him. "What I would like to emphasize," Remini argues, "is Jackson's right to many of the claims advanced for him by the Progressive and New Deal historians. He was in fact a Man of the People." Old Hickory's commitment to the American majority of workers, farmers, and the producing classes was unambiguous and total. He "insisted on democratic rule. . . . Only the people—no one else—had the final say." Above all, "majoritarian rule was the only thing that mattered in his thinking about the operation of government." Thus, "under Jackson the concept of popular or majority rule really took hold. . . . More than any other single individual he contributed to and symbolized the arrival and acceptance of that concept. His charisma, popularity, and accomplishments made it all possible" (pp. xv, 25, 315, 337).

Clearly, Remini expresses few doubts. His determination to redress what he considers the imbalance of recent scholarship is clear and sharply etched. Those who have strayed into different channels get their lumps—Richard Hofstadter and Bray Hammond about entrepreneurial forces and bank policy, Michael P. Rogin and Edward Pessen about motivation and democratic commitments, and the new political historians about who was for whom and why. Arthur Schlesinger and Alexis de Tocqueville return to center stage along with James Parton. There are, to be sure, occasional criticisms, but these are of little importance given the general thrust of celebration, deference, and democratic accomplishment.

Of course, despite the scholarly industry, imposing detail, and articulately expressed imagination, there are those who will find Remini's judgments and focus incomplete and too dismissive of ambiguity and contradictions, a distortion of a complex era. Too much scholarly energy has suggested otherwise. Jackson never wrote his autobiography, and the art of campaign biography developed after his retirement. Yet one comes away with the feeling that here is how Jackson saw himself, might have set forth his own case, and wished to be remembered.

JOEL H. SILBEY
Cornell University

MAJOR L. WILSON. *The Presidency of Martin Van Buren*. (American Presidency Series.) Lawrence: University Press of Kansas. 1984. Pp. xiii, 252. \$17.95.

Shortly after his reelection defeat in 1840, President Martin Van Buren predicted, "Time will unravel the means by which these results have been produced, and the people will do justice to all." At last Van Buren's time has arrived. New biographies by John Niven and Donald Cole chart the political career of the man known to his contemporaries as the "Red Fox of Kinderhook." Broadening an earlier assessment by Robert Remini, these books show Van Buren's intuitive grasp of political realities and the critical role he played in the formation of the Democratic party. The volume under review is the second study of Van Buren's presidency to appear in little more than a decade. Writing for the American Presidency Series, Major L. Wilson shares with modern Van Buren scholars the belief that the eighth president was very much his own man and not merely a caretaker for his strong-willed predecessor, Andrew Jackson.

Wilson begins his study with a perceptive albeit disorganized overview of Jacksonian society. Quickly focusing on the interaction of economic expansion and political development, he charts the growth of the Democratic party through the election of 1836 and then doubles back to analyze Van Buren's rise to power. In these opening chapters, Wilson alternates between incisive analysis of party structure and pedestrian narrative of well-known events. This confusing structure obscures key issues such as the personal and political relationship between Van Buren and Jackson.

Once into Van Buren's presidency, Wilson takes full command of his material by concentrating on the Panic of 1837. The author provides a masterful account of the banking practices and federal policies that precipitated this economic upheaval. Wilson deftly incorporates the findings of recent economic histories that view the panic not as a cataclysmic depression but merely as "an interruption" in a

"cycle of expansion that had begun in 1830" (p. 55). Although solidly grounded in econometric theory, this line of argument shows little appreciation for the sense of political emergency that pervaded Washington in 1837 and forced Van Buren to call a special session of Congress.

Wilson's next four chapters provide a logical and comprehensive account of the president's three-year campaign for congressional enactment of an independent treasury. With admirable clarity the author explains treasury monetary policy, state banking practices, and congressional legislation. Yet he does not calculate fully the political cost of the independent treasury. By proposing legislation to divorce the fiscal interests of the federal government from those of the states, Van Buren struck at the very core of states' rights philosophy on which he had founded the Democratic party. Losses in the key Democratic strongholds of New York and Virginia presaged his own defeat in 1840.

Wilson writes briefly of foreign affairs during Van Buren's presidency. His assessments of the Canadian border rebellion, the Mexican claims dispute, the Aroostook War, and Texas's bid for annexation ably synthesize previous scholarship. Wilson's penultimate chapter is badly titled "Running the Shop" and may turn away readers who will assume that it deals with routine administrative history. Tucked away in this section is a sensitive discussion of Indian affairs that deserves recognition. Smooth narrative though it is, the final chapter on the election of 1840 provides little explanation of the Democratic defeat and no adequate summary of Van Buren's career as either party theorist or president.

JAMES C. CURTIS
University of Delaware

WALTER L. BUENGER. *Secession and the Union in Texas*. Austin: University of Texas Press. 1984. Pp. 255. \$17.50.

This traditional narrative account of the secession movement in Texas is buttressed with an intellectual analysis of the changing meaning of nationalism for the citizens of that southwestern state. The author has raised the classic Civil War repressible/irrepressible conflict theme and appears to join the repressible conflict advocates. Walter L. Buenger's detailed account of how Texas's leaders succumbed to popular pressures also places him in the "died of democracy" school. His study of a populace that no longer trusted the Union gives plausible reasons why southern nationalist sentiment arose. The conclusion shows a divided Texas unite in a search for order and harmony, despite the seemingly divisive revolutionary act of secession.

The strongest sections of this monograph reveal how opposition to secession eventually was quashed in precisely those areas where opposition had been viable and plausible. On the frontier, the area least tied to the Old South, fears that the Union could no longer protect the people from the Indians led the settlers to look to another nation for support. Immigrant Germans, previously thought to have been thoroughly unionist, also eventually provided support for secession or at least did not organize resistance to it. As outsiders they felt a keen loyalty to the idea of nation, so when the secessionists convinced a number of them that support of the American government was detrimental to their interests, they approved the creation of a new nation. Even the Mexican-Americans, who had no love for the Anglos, did not oppose the public demand for separation. In fact, only those parts of the state with no ties to the South or need for the nation gave support to the Union.

Despite this careful overview of division and unity in Texas, the book is weakened because it relies too much on traditional sources of analysis, especially newspapers, which do not really test the state's divisions and its true feelings for the nation. Other studies of the election of 1860 and its results make a more rigorous comparison between the religious beliefs of immigrant Catholics and Protestants. Despite the author's assertion that Texas Catholics owed much to the Democrats for opposing the Know-Nothing party, there is no attempt to test just where the immigrant group stood on secession. Moreover, there is little comment on just how the idea of nation translated into loyalty to government, let alone an understanding of the difference between nationalist ideology and the practical activities of government. Thus, the author propounds a schematic continuum in the public conscience of loyalty to the nation that concludes with Calhoun as a symbol of the new southern nation. Only because the author takes seriously the editorials in secessionist newspapers and has no way really to gauge public opinion and perceptions can such false imagery emerge.

The contributions and faults of this book call for even further questions on just how a Texas so divided along economic, ethnic, and attitudinal lines could possibly unite. It suggests that more careful study of the differences among Texans could show that they acted not out of unity, but precisely because they were so disorganized and diverse. Popular feelings, which certainly controlled political leaders, were divided to the point where actions may have resulted from fear of further divisions. Also, the nationalist theme, when listened to carefully, offers the song of states' rights. One should look more closely at heterogeneity and the origins of

pluralism, themes equally as modern as nationalism, to find the disharmony in the South.

JON L. WAKELYN
Catholic University of America

MICHAEL BARTON. *Goodmen: The Character of Civil War Soldiers*. University Park: Pennsylvania State University Press. 1981. Pp. 135. \$14.95.

Michael Barton is concerned with the character of Civil War soldiers, defining the "goodman" as one who had his passions well under control. In what he defines as a "quantitative, sociolinguistic case study in historical psychological anthropology," Barton seeks to discover "how that character theory was practiced by over 400 men during the Civil War, if only in the form of their verbal behavior in diaries and letters" (p. 4).

After a brief survey of historians' varying descriptions of northern and southern character, Barton tests the consensus that despite common values, northerners tended to be more "Puritanical" and willfully controlled" than romantic, less emotionally controlled southerners (p. 21). Examining sixteen core values such as moralism, progress, achievement, and the like, he finds that in some respects soldiers on each side shared the same values, but, in regard to religion, patriotism, efficiency, and individualism, northern enlisted men differed significantly from northern officers and from southern enlisted men. The greatest contrast was between northern enlisted men and southern officers, who stood poles apart. Similar results were found in an analysis of eleven character values, such as "honorable," "moral," "kind," and "dutiful." Southerners evaluated character much more frequently than did northerners. Again the greatest difference was between southern officers and enlisted northerners, although southern officers, southern enlisted men, northern officers, and northern enlisted men ranked both core values and character values in much the same order. By any measure, including writing style, it seems that Confederate officers were significantly more expressive than their enlisted counterparts in the Union army; they were "model Americans," differing from enlisted northerners, who Barton characterizes with a statistical adjective as "modal Americans" (p. 41). Southern enlisted men and Union officers fell between the two extremes. Barton attempts to explain these differing behavior patterns by reference to theories of economic and technical development, child rearing, race relations, and sex-role differences. He appears unsure of the strength of his explanations, but contends that the interrelationship of black and white in southern life is a large part of the cause for differing sectional character. Ambivalence toward

blacks created a southern character that possessed both restraint and release, whereas northerners had no option; for them, it was simply a question of keeping control. Northerners were indeed more willfully controlled than southerners.

Barton's study is a model of the New History; it is difficult to do justice to this tightly argued, logically developed analysis. One will have to go far to find other work in American history that employs quantitative content analysis so effectively and lucidly; Richard L. Merritt's work twenty years ago on the development of American nationalism during the colonial period comes immediately to mind. There are only a few misgivings. Coders did not cross-check each other's work, though Barton himself sampled it. The available documents overrepresent educated and professional men, although it is difficult to see how this problem could have been avoided; given the brevity of the book, more traditional evidence in the form of illustrative quotation would have been appropriate.

To historians who might be tempted to say Barton's findings are not that new, the reply is that we now have a more precise notion of cultural distinctiveness than we had before.

RICHARD E. BERINGER
University of North Dakota

JAMES LEE MCDONOUGH and THOMAS L. CONNELLY. *Five Tragic Hours: The Battle of Franklin*. Knoxville: University of Tennessee Press. 1983. Pp. 217. \$17.50.

Battles are back as suitable subjects for serious scholarly study. In recent years, about a dozen Civil War battles have been recounted and analyzed in major books by professional military historians. One such study is *Five Tragic Hours: The Battle of Franklin* by two prominent scholars of the Confederacy: James Lee McDonough, historian of earlier battles at Shiloh and Stone's River, and Thomas L. Connolly, historian of the Army of Tennessee.

This slim book summarizes the southern advance from Georgia into Middle Tennessee in the autumn of 1864, closely examines the Spring Hill affair on November 29, and outlines the disastrous Battle of Nashville and the ensuing flight from Tennessee in December. Attention concentrates on the Battle of Franklin on November 30, where General John B. Hood's Confederate Army of Tennessee was bloodily repulsed (with a loss of 12 generals, 54 regimental commanders, and 7,000 men) in a series of disconnected frontal assaults against General John M. Schofield's entrenched Federal army. The authors portray the battle through a judicious mix: analyzing generalship, recounting division- and bri-

gade-level operations, and quoting enlisted and junior officer participants.

The book's strengths—and they are many—lie in several areas. McDonough and Connelly have worked so extensively on so many facets of the Civil War in Tennessee that they can ground this book in vast (if not quite exhaustive) knowledge of published and manuscript sources. The authors, moreover, persuasively delineate (though they do not resolve) the contradictory and confusing aims and accounts concerning Spring Hill. Persuasive, too, are their assessments of the generalship and character of Hood and Schofield. And their main battle narrative succeeds in the difficult task of specifying the tactics, conveying the humanity, and evoking the horror of that terrible afternoon and evening at Franklin. They show that, in a very real sense, Franklin marked the death of the once great Army of Tennessee.

Yet the book also has shortcomings—and, unfortunately, they too are many. Some are typographical, others linguistic, still others historical, pertaining both to Franklin and to other parts of the war. Historical errors on Franklin include confusing Robert and Gordon Granger (p. 22), calling *Colonels* Jacob Biffle and George Dibrell generals (pp. 88, 153), putting James Holtzclaw's Brigade in Carter L. Stevenson's Division and Charles Shelley's Brigade in Edward Johnson's Division (pp. 156, 206), and claiming that Johnson had three brigades, only to acknowledge in the very next paragraph that he really had four (p. 150). But the cause for most serious concern is the repetition of the same full names, the same phrases, the same quotations, the same sources, the same descriptions in succeeding chapters, succeeding pages, sometimes even succeeding paragraphs. The resulting narrative is at times as disjointed as the attacks that it recounts. One surmises that each author wrote distinct but sometimes overlapping sections from the same sources and that neither editor nor authors smoothly merged those disparate parts. Eliminating such duplication would have greatly enhanced this book's readability.

Despite those shortcomings, the book's value remains its tactical narrative and its analyses of generalship. East and West, North and South—it was all one war, and our understanding of that war is the richer for this latest contribution by McDonough and Connelly.

RICHARD J. SOMMERS
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ALAN A. SIEGEL. *For the Glory of the Union: Myth, Reality, and the Media in Civil War New Jersey*. Cranbury, N.J.: Associated University Presses, for

Fairleigh Dickinson University Press. 1984. Pp. 231. \$29.50.

This is a curious book; one might even call it a non-book. The vague subtitle appears to promise some sort of analysis of the role of the New Jersey press in reporting the Civil War. Indeed, Alan A. Siegel lays the foundation for such an analysis when he reminds us in his preface that "partisanship" and "prejudice" ruled the Civil War press and that newspaper accounts of campaigns and battles often varied widely, depending on the politics of the journals. This conclusion is hardly new. Anyone who has read J. Cutler Andrews's books, *The North Reports the Civil War* and *The South Reports the Civil War*, already knows that such partisanship was endemic to the press on both sides of the Mason-Dixon line. Nor is a separate study of New Jersey's press in this period warranted. The author himself confesses that "none of New Jersey's papers was in the same league as the great metropolitan journals of the day" (p. 33).

Why, then, did Siegel choose to do a study of New Jersey's press in the Civil War period? The answer is that he did not. After writing a gratuitous general essay on New Jersey's newspapers, the author tells us that he will concentrate on just three Newark journals—the *Daily Advertiser*, the *Journal*, and the *Mercury*—because they reflected and typified the viewpoints held by the state press. Nor does the narrowing of scope end here. The bulk of the book concerns the Newark newspapers' reports concerning the activities of the Twenty-sixth New Jersey Regiment, a unit of nine-month volunteers organized in 1862, largely by those who wanted to avoid the draft. This outfit's undistinguished beginning was matched by its lackluster record during its period of active duty from September 1862, through June 1863. The regiment was almost never used in combat, and when it did see action, as at Chancellorsville, its troops broke and ran under fire. Even the author concludes, "The regimental career was brief, awkward, and of little significance to the Union war effort" (p. 62).

The book's value is further diminished by Siegel's treatment of his subject. After his introductory chapters, the author drops his analysis of newspapers for the most part and devotes the remainder of his pages to soldiers' accounts of the regiments' experiences on campaign. He strings together extensive quotations, sometimes running two full pages in length, with a few connective sentences of his own. Although these quotations are often interesting and sometimes amusing, they generally have nothing whatsoever to do with "Myth, Reality, and the Media."

In short, Siegel, who is an amateur historian and—not incidentally one suspects—the great-

grandson of a member of the Twenty-sixth New Jersey, has actually written a tolerably interesting little history of an insignificant and short-lived Civil War regiment. Unfortunately, he has tried to represent it as something more and in the process has garbled and thus weakened the final product. Other descendants of the members of the Twenty-sixth Regiment and perhaps the most dedicated New Jersey Civil War buffs may want to examine the book, but historians will find it largely a waste of time.

DONALD E. REYNOLDS
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JOSEPH B. JAMES. *The Ratification of the Fourteenth Amendment*. Macon, Ga.: Mercer University Press. 1984. Pp. viii, 331.

Nearly thirty years ago, before the efflorescence of revisionist scholarship on Reconstruction, Joseph B. James's *The Framing of the Fourteenth Amendment* carefully analyzed the political motives behind the formulation of the most important of the three Civil War constitutional amendments. James's account still stands as a useful corrective to interpretations of Reconstruction that perhaps exaggerate the role of the constitutional principle or humanitarian impulse in Republican party actions.

Intended as a sequel to the author's earlier work, *The Ratification of the Fourteenth Amendment* is a detailed account of the actions of the states in approving or rejecting the central feature of the Republican Reconstruction program of 1866. Observing idealism, pragmatism, and sectionalism at work in both the North and the South, James states that southerners rejected the Fourteenth Amendment in 1866-67 because, given the choice, they preferred not to cooperate in their own humiliation. A moment of opportunity existed in 1866, James writes, when peaceful restoration could have been achieved if those in positions of responsibility had worked harder for a unified nation. But mutual distrust of political motives caused the opportunity to be lost, and eventually the South accepted the Fourteenth Amendment as the necessary price of restoration.

James offers these generalizations not as a sustained argument or interpretation but as occasional reflections in the course of presenting a careful chronicle of the ratification process. The main value of the book, then, is not to provide a new view of Reconstruction, but rather a useful compendium of information on the actions of the state legislatures on the Fourteenth Amendment.

HERMAN BELZ
University of Maryland

MARK W. SUMMERS. *Railroads, Reconstruction, and the Gospel of Prosperity: Aid Under the Radical Republicans, 1865-1877*. Princeton: Princeton University Press. 1984. Pp. xiii, 361. \$37.50.

By the First Reconstruction Act of March 3, 1867, Congress replaced the presidentially-mandated governments in the former Confederate states with military governments and instructed the military commanders to create new electorates in each of the states, composed of former black slaves and such whites as could take an oath attesting past loyalty to the Union. Thus, Congress enfranchised the former slaves and disfranchised a large part of the former political leadership. This step was intended to allow the Radical Republicans to take over the southern state governments, and generally they did just that. The incapacity and corruption of the new governments became notorious and was an important factor in leading to their eventual undoing. With the adoption of the Amnesty Act in 1872 and with the effects of the depression of 1873-78, a new combination of conservatives, whom C. Vann Woodward has labeled the "Redeemers," took control in the mid-seventies. In their appeals to the public the Redeemers represented the Radical Republicans as a venal and depraved lot, and historians subsequently took the same view.

In recent years a new generation of historians has been disposed to examine the Radical regimes more sympathetically. Mark W. Summers concentrates in the present volume on the issue of state aid to railroads and on the Radicals' views of themselves and of their own actions. As opposed to the historians of the earlier Dunning school, he slights conservative newspapers and studies carefully the Radical ones. The result is perhaps not a balanced picture, but it deals with the Radicals in their own terms and brings to light a side of things formerly neglected. The Radicals emerge as real, practical people, often inept, corrupt, and easily bribed but nevertheless dedicated to the public welfare as they saw it. The author sometimes uses whitewash, but he also applies some harsh names to the Radicals—and then says they were much like the Democrats. In all this he makes some good points. He is not carried away by bias, except, perhaps, that which is inherent in his approach. He believes the railroad aid policy was a critical element in the ultimate failure of the Republican party in the South.

He speculates that, if the Conservatives had been in power in the Radical period (he uses the terms "Democrats" and "Republicans"), they would have done about the same things as the Radicals. The reviewer finds something missing in this: the Radicals supported local agricultural, commercial, and railroad interests that were not served by the prewar railroads or that clashed with them. The differences

that appeared in the legislatures were often subtle, however, and individuals might have been influenced to change their minds.

The author has been a diligent researcher, but the book is prolix, redundant, and full of vague and authoritative pronouncements that are not supported by the citations lumped together at the ends of the paragraphs. His acquaintance with the economic characteristics of railroads and with their pre-Civil War history is slight. He deals with a complex pattern of affairs with which he is not fully prepared to cope. But who is? May he continue his efforts.

JAMES F. DOSTER
University of Alabama

JOHN LAURITZ LARSON. *Bonds of Enterprise: John Murray Forbes and Western Development in America's Railway Age*. Foreword by ALFRED D. CHANDLER, JR. Cambridge: Division of Research, Graduate School of Business Administration, Harvard University; distributed by Harvard University Press. 1984. Pp. xvii, 257. \$27.50.

John Lauritz Larson has written a book of global reach, stretching from Canton, China, to Denver, Colorado—the long way around. John Murray Forbes, the Boston merchant-cum-railway investor is the story's protagonist, a man who traveled with all the baggage of his merchant upbringing into the railway age and ironically set in motion forces that fundamentally altered the very conditions that created him. A man equally at home among Bernard Bailyn's seventeenth-century merchants and among nineteenth-century industrialists, a man who preferred to trust relatives to carry on his far-flung businesses, a man who acted only after great deliberation, Forbes also risked his capital in Asia and on the Michigan, Illinois, and Iowa frontiers. He was a foreigner in all of these areas; he was ever the intruder bringing change in his wake.

The Boston investor is simply a vehicle, however, that Larson uses to examine the effect of the railroad in newly settled Iowa. Forbes always believed that railroads should be laid to exploit the trade of developing regions, whereas westerners were equally certain God sent railways to create new economic opportunities. Local Iowa projects sprang up everywhere like June corn, and almost as quickly their backers had to go east to beg financial help. The next step was predictable; eastern capitalists such as Forbes took over their roads. This crushing realization, driven home in Iowa in the 1857 depression, set outside investors against local customers for the first time. Easterners were angry that farmers refused to ante up more money for their projects, whereas Iowa yeomen were convinced that East

Coast bankers had stopped construction just to squeeze further subscriptions from financially strapped agriculturalists. Larson skillfully uses these regional conflicts to set up the postwar political and economic struggles that pitted East against West, farmer against the industrial world, and the haves against the have-nots; the railways, he argues, made it all possible.

Disgruntled Iowans struck back the only way they knew—politically. And Larson shows how Forbes's struggle with his Iowa friends paralleled tendencies in the nation's capital. The grangers demanded local political control of improvements, while Forbes, the old free trader, aligned his economic interests with the oncoming political centralization. The most reasonable solution, Larson intimates, was federal regulation, a step that did not unduly rile Forbes. The book abounds in such ironies: Forbes invested in railways to bring his countrymen together but only prompted angry sectional quarrels; dedicated to minimal government participation in economic affairs, he welcomed federal railway regulation so that his roads would have greater freedom.

This book is reminiscent of Richard Overton's *Perkins/Budd* in that Larson has deftly used Forbes and his ideas to show a response to industrialism in the United States. At the same time he has very neatly enlarged our view of the response of Robert Wiebe's consensual communities after railroads encroached on their preserves. Further, Larson indicates that local reactions from the first were anything but passive. Farmers in Iowa had no qualms about challenging Forbes; they only lacked the financial resources to work their wills. Larson has admirably worked his in this most readable book.

JAMES A WARD
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Chattanooga*

RAYMOND ARSENAULT. *The Wild Ass of the Ozarks: Jeff Davis and the Social Bases of Southern Politics*. Philadelphia: Temple University Press. 1984. Pp. xv, 336. \$34.95.

Historians have failed to understand the phenomenon of early twentieth-century southern demagoguery, Raymond Arsenault contends, because they have focused too much on the personalities and shenanigans of individual leaders and too little on the responses of voters. From 1898 to 1912, as an Arkansas attorney general, governor, and U.S. senator, Jeff Davis's mass base was rural and was attracted to him, Arsenault speculates, by a desire to resist the modernizing culture of towns and cities and its symbolic instrument, the railroad. Whereas in the 1880s and 1890s, Arkansas Wheelers and Populists had gained near-majority popular support

for significant economic and social change, the gentry-baiting, Yankee-baiting, race-baiting Davis, who filled the political air with irresponsible charges about his opponents but never carried through on any judicial, executive, or legislative program, offered the hicks only a "politics of catharsis" (p. 15), a rhetorical denigration of their geographically or racially distinct adversaries. Less neatly separated into politically relevant social groups than their ethnically and religiously divided northern counterparts and largely protected, as a result of disfranchisement, against racial and partisan schisms, voters in the disorganized and restricted one-party Arkansas electorate, Arsenault conjectures, were skeptical of governmental activism and were consequently well satisfied to attend Davis's breadless circuses.

The son of a prosperous small-town lawyer, real estate agent, and railroad promoter whose devotion to the Confederacy did not extend to volunteering to fight, Jeff Davis attended college and law school in Arkansas and Tennessee, married within the local gentry, became a loan agent for a cotton factor, and dressed during the 1880s not like the one-gallused farmer that he later celebrated in harangues, but as the vested booster he actually was at that time. A bitter opponent of the Republicans, Wheelers, and Populists, Davis staunchly backed the poll tax and secret ballot "reforms" that decimated those parties, especially their black supporters. Moving safely within William Jennings Bryan's shadow, Davis won a plurality Democratic nomination for state attorney general in 1898 by a stroke of luck, when the leading candidate died of apoplexy. The shift from a convention to a primary method of nomination enabled Davis, whose rough-and-tumble antics amused the crowds, to parlay his quickly abandoned, largely sham battles against out-of-state trusts and an expensive new state capitol building into a overwhelming gubernatorial victory two years later. As governor and senator, he preferred drinking, stump speaking, and hunting and fishing (in approximately that order) to working for or executing laws, so he left no legacy of accomplishment. The judgments of Arkansas's leading Populist, who thought Davis an unprincipled charlatan, and of a northern newspaper, which dismissed him as "explosive but not dangerous," seem to this reader, although not always to Arsenault, precisely on the mark.

Davis's coalition was not class based, the author's statistics show conclusively, nor was it as stable as Davis asserted or as Arsenault sometimes treats it. Indeed, the chief failing of Arsenault's book is that he never attempts to construct one or more explicit and comprehensive statistical models of the pro- and anti-Davis voter alignments. The large number of bivariate correlations that he presents—data aggregated at the township and county levels lead to

strikingly similar conclusions—implies that no multiple regression model would be very satisfactory. Davis's support varied widely from election to election (appendixes A and B) and could not be transferred to other candidates (pp. 108–09, 235, 238), and in only 3 out of 250 cases do social or economic variables explain more than 25 percent of the variance in the Davis vote (appendix A).

Although Arsenault at times tries to picture his central figure as something more, his evidence demonstrates that Davis was just a clever, besotted ranter, a somewhat extreme product of a primary system in a one-party polity with a restricted electorate. It is little wonder that this shifty figure attracted only shifting support. Although his findings are largely negative, Arsenault's clear and pleasantly written monograph advances the study of southern demagoguery to a new plane of self-consciousness and sophistication.

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ROGER D. MCGRATH. *Gunfighters, Highwaymen, and Vigilantes: Violence on the Frontier*. Berkeley and Los Angeles: University of California Press. 1984. Pp. xvi, 291. \$16.95.

This book about two mining towns that straddled the Sierra between California and Nevada is billed as "the first truly representative look at violence on the frontier." To quote further from the dust jacket, it "strips away the facade of violence." Previous scholarship is toppled, and widely accepted fact is shown to be myth.

In his preface, Roger D. McGrath makes the dangerous claim that he has "exhausted every available source." Yet his bibliography lists only two nongovernmental manuscript collections, contains no oral history sources, and does not include Charles H. Shinn's vintage book, *Mining Camps: A Study in American Frontier Government*, or Duane A. Smith's insightful *Rocky Mountain Mining Camps: The Urban Frontier*. McGrath does use Mark Twain's wonderful but factually suspect *Roughing It* (but not Twain's blast therein at trial by jury). Neither is McGrath skeptical of sometimes outrageous newspaper stories. More importantly, some of his conclusions are hard to swallow, especially his unprovable statement about Bodie, California. "Chinese, Mexicans, Indians and blacks, then, never experienced any violence directed at them merely because they were members of minority groups" (p. 148). Readers familiar with frontier mining towns other than Bodie, California, and Aurora, Nevada, may balk at some of McGrath's claims, including his Bodie chapter title, "The Last of the Old-Time Mining Camps."

McGrath's writing is lively and readable. Readers are treated to numerous drunken sprees, lynchings, Indian fights, brutal beatings, abused women, and murders galore—more than enough for a Hollywood contract.

This is an excellent study of Bodie and Aurora, but the limited data available about these two towns does not securely buttress broad conclusions about the frontier in general. Only in his final chapter and in an appendix, "Scholarly Assessments of Frontier Violence," does McGrath begin effectively to convince readers that, at least on one mining frontier, "the old, the young, the unwilling, the weak, and the female—with the notable exception of the prostitute—were, for the most part, safe from harm. If, as many popularly assume, much of America's crime problem stems from a heritage of frontier violence and lawlessness, then it is ironic that the crimes most common today—robbery, theft, burglary, and rape—were of no great significance . . . on the trans-Sierra frontier" (p. 247). McGrath does admit that the frontier homicide rate was higher than the national rate, then and now.

This book should be of interest to a broad range of scholars. The thirteen chapters include detailed discussions of mining-town origins, removal of the Piute Indians, impact of boundary disputes and the Civil War, vigilantes, violence and minorities, violence and women and children, and a quantitative comparison of frontier and urban violence. McGrath challenges the traditional view of frontier violence advanced by such scholars as James Truslow Adams and, more recently, Joe B. Frantz. Despite some reservations, McGrath agrees with revisionists, such as Robert R. Dykstra and Eugene V. Hollon, who find the frontier a wholesome, tranquil place. Frederick Jackson Turner would be pleased.

"Frontier violence" may be overemphasized, McGrath points out, because it "has infinitely greater appeal to the reader than frontier calm" (p. 270). This book should advance the revisionist view of a duller, safer frontier—if you pardon a lot of drunken pandemonium, "justifiable" homicides, "socially constructive" vigilante lynchings, and the violent disposition of nonwhites.

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GAINES M. FOSTER. *The Demands of Humanity: Army Medical Disaster Relief*. (Special Studies.) Washington: Center of Military History. 1983. Pp. x, 188. \$5.00.

In recent years, military historians have shown increasing interest in the noncombatant roles of the United States armed forces: their involvement in

science, exploration, civil works, and domestic disorders. Published by the army's Center of Military History, Gaines M. Foster's survey of the role of army medical personnel in disaster relief reflects this trend. While Foster emphasizes the contributions of the medical department, he also provides the best overview yet of the army's general response to natural disasters, both at home and abroad.

Through the nineteenth century, army relief to the victims of earthquakes, floods, epidemics, and other natural disasters was sporadic and *ad hoc*, limited by traditions of localism and by congressional reluctance to appropriate funds. The imperialistic thrust at the turn of the century brought the army's first—and best-known—major relief mission: successful public health campaigns conducted by medical officers to control disease in the Philippines, Cuba, and Puerto Rico. During the early twentieth century, the medical department's relief role expanded. Urbanization increased the damage and suffering caused by disasters, local civilian relief agencies lacked the resources to meet the need, and the Progressive era's emphasis on efficient, centralized management and bureaucratic solutions to social problems removed the ideological barriers to army involvement. After World War I, army medical units participated in the Wilson administration's efforts to combat famine and disease in Poland and Russia, missions motivated by both humanitarianism and the desire to limit the spread of Bolshevism. The interwar years brought friction between the army and expanding civilian relief agencies, especially the American Red Cross. The medical department's involvement in domestic emergencies faded after World War II, as the Red Cross assumed primary responsibility and the federal government defined its main role as long-term rehabilitation rather than immediate relief. Ironically, military relief in foreign disasters expanded after 1945; governmental leaders perceived such missions in the context of containment—as a means to build "good will" and increase American political and economic influence in developing regions of the world.

Foster bases his study on extensive research in official army records. He includes descriptions of all of the major relief efforts as well as an overview of the evolution of federal disaster policy. Despite his role as an "official" historian, he is by no means uncritical of the army's relief performance. He finds that medical personnel were culturally myopic at times, recognizes that relief efforts were often marred by poor planning and lack of coordination, and stresses the central role of political self-interest in foreign disaster aid. Nevertheless, he considers the medical department's overall effort to have been valuable, filling a gap until the development of adequate civilian organizations and providing military personnel with practical peacetime experience

in handling large-scale emergencies. The only significant flaw in this work is one of focus and is probably inevitable given the subject. Medical relief was only a part of the army's broader relief role, and Foster's emphasis on the medical department at times exaggerates its relative importance to the general effort.

WILLIAM B. SKELTON
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Stevens Point

WILLIAM F. WILLINGHAM. *Army Engineers and the Development of Oregon: A History of the Portland District U.S. Army Corps of Engineers*. Washington, D.C.: Government Printing Office. 1983. Pp. xiii, 258. Cloth \$20.50, paper \$12.50.

The prehistory of the Army Corps of Engineers' activities in the Pacific Northwest began with the explorations of Meriwether Lewis and William Clark and extended through the building of military wagon roads in the 1850s. In 1866 a "Rivers and Harbors of the Pacific Coast" office was established in San Francisco, and finally in 1871 a separate Portland Engineer Office was formed.

William F. Willingham divides the 110-year history of the Portland District into three eras. First and longest, extending through the 1920s, was the period during which the improvement of navigation was the overwhelming object of the corps. The Columbia and Willamette rivers were dredged and otherwise "improved," and harbors along the Oregon coast were similarly upgraded, often through the building of jetties. Beginning in the 1930s, a new responsibility for flood control and interest in hydroelectric power were made part of plans for "multipurpose" river development. Finally, beginning in the 1960s, a national demand for a larger measure of environmental quality led to new regulatory responsibilities for the corps and a greater emphasis on integrated, regional planning.

Willingham states in his preface that his purpose was to "show and explain the causes and consequences of this transformation of mission" within the "context of the economic growth and development of the Pacific Northwest" and thereby to tell the "story of men and women challenging and transforming the natural environment to make it serve man's needs" (p. ix).

This book is the latest in a series that eventually will include a history of each of the districts and divisions of the U.S. Corps of Engineers. The corps has a historical division, housed within the Office of the Chief of Engineers and staffed by professional historians, but these discrete histories are contracted for by the individual districts. The contract historians are given wide access to corps archives, and the

historical division exercises what influence it can to maintain professional standards in the face of whatever understandable hope there may be at the local level to be painted in a favorable light.

Yet somehow these district histories, although presenting a wealth of accurate information not otherwise easily available, continue to evoke a vague disappointment. In part, this disappointment may result from the unidimensional view that emerges when little more than agency sources are consulted. It may also derive in part from the common practice of merely accepting the Whiggish perspective of the principal actors, those who see their mission as "transforming the natural environment to make it serve man's needs."

This book suffers from both these constraints. It is best when covering the early years, when navigation was the corps's principal goal, and becomes less satisfactory when it deals with the last fifty years—not because of censorship or a lack of records, but no doubt because of the very abundance of records dealing with an increasingly complex mission and a natural tendency to begin to see the world in those same terms that inform the sources one uses.

This book therefore is a well-written and lavishly illustrated account of what the Portland District of the corps has been up to for the past century. It should be thought of as a beginning to, not a summation of, the causes and effects of that activity.

CARROLL PURSELL
University of California,
Santa Barbara

G. J. A. O'TOOLE. *The Spanish War: An American Epic—1898*. New York: W. W. Norton or Stoddart, Don Mills, Ontario. Pp. 447. \$19.95.

Standing in the mainstream of recent historiography, G. J. A. O'Toole's *The Spanish War* takes the conflict of 1898 seriously and does not consider it as a bizarre, ridiculous episode. In this regard the author closely follows David F. Trask's superb *The War with Spain*. Like Trask, he recognizes McKinley's sincere desire to maintain peace, the significance of domestic pressures in both Spain and the United States on the decision for war, the bravery of both Spanish and American soldiers and sailors, and the Cuban revolutionaries' invaluable contribution to American success. The only new interpretation O'Toole presents concerns the *Maine*. He argues that fear of German meddling in the Caribbean was as important as the imbroglio with Spain in prompting the McKinley administration to send the ship to Havana. Although the dust jacket proclaims that "the author unearths a previously unknown U.S. intelligence network in Cuba during the war," this is not true, since Trask had already

noted the existence of a spy in Havana. O'Toole merely identifies the spy and provides details about his operations.

It appears, however, that O'Toole's primary interest is not in sustained analysis and new interpretations, but in good storytelling. Indeed, for sheer reading pleasure, this book is hard to beat. The author displays a novelist's eye for the human-interest aspects of history (which is not surprising since he has published two historical novels), emphasizes personalities and dramatic incidents, and relies on vignettes and anecdotes to carry the narrative forward. People and events that warrant, at most, a cursory mention from Trask get much fuller treatment from O'Toole. Winston Churchill's visit to Cuba in 1895, Frederick Funston's activities with the Cuban revolutionaries, Lieutenant Colonel Joseph E. Maxfield's balloon during the Battle of San Juan Heights, the wartime exploits of Richard Harding Davis, Stephen Crane, and other correspondents—each of these (and many more!) benefit from several pages of sparkling prose interspersed with well-chosen quotations from primary sources.

Unfortunately, serious defects detract from the book. Factual errors are frequent. These include placing the First Marine Division at Guantánamo (no such unit then existed) and locating Guánica on Puerto Rico's southeastern coast (it is on the southwestern coast). Surprising omissions occur. For example, O'Toole does not mention Adjutant General Henry C. Corbin, who was McKinley's unofficial chief of staff, or the Eastern Squadron, which had an important strategic impact. Some significant events are inadequately covered. The discussion of American manpower mobilization demonstrates little understanding of the differences between the regular army, the National Guard and state volunteers, and federal volunteers; O'Toole cites Graham A. Cosmas's *An Army for Empire* in his bibliography, but one wonders how carefully he read it. The account of the Philippine-American war resulting from American acquisition of the Philippines emphasizes only the struggle's ugly features. Significantly, O'Toole's bibliography does not list John M. Gates's *Schoolbooks and Krag's*, which discusses the benevolent aspects of American rule.

These shortcomings are distressing, since they mar an otherwise delightful book. Although professional scholars will still turn to Trask, they might recommend O'Toole to undergraduates and general readers—but only after issuing suitable caveats.

PETER MASLOWSKI
University of Nebraska,
Lincoln

ELLEN NORE. *Charles A. Beard: An Intellectual Biography*. Carbondale: Southern Illinois University Press. 1983. Pp. xii, 322. \$24.95.

Ellen Nore in this revised Stanford dissertation confesses too modestly in the preface (p. ix) that she has "not really succeeded in calling Beard back in an immediate way." The fact is that she has written the fullest and most knowledgeable study of the man and his work that we have had to date. In addition to using several hitherto unexploited manuscript collections, she has read widely in the writings of Beard's contemporaries and associates and in the huge corpus of commentary about him. Her book is therefore a close view of the immediate circumstances and proximate influences that shaped Beard's total output—the textbooks, the tracts, the philosophical essays, the reports on contemporary problems, and the major works. This detailed contextualization, sympathetic though not uncritical, recaptures much of the immediacy of Beard's experience and, in doing so, counters a good many charges leveled at him by less well-informed critics. Young scholars on the left should find in Nore's Beard an intellectual legacy of surprising relevance to their own concerns.

Perhaps the freshest chapters are the early ones on Beard's youthful involvement with the English working class and on his career as a municipal planner. One learns, for example, that a political scientist in the Progressive era did not *have* to be an elitist in striving to make government efficient and businesslike. Beard thought that democracy and efficiency were reconcilable because clear-cut procedures would enable the public to understand and thus to oversee what the experts were doing. As a historian and a national planner, Nore points out, he was a materialist though not a Marxist, believing in the driving force of wealth and technology. Beard's historical relativism is construed not as a tissue of contradictions, or as a repudiation of science, but as a logical development from the New History under the influence of a New Physics. Similarly, Beard's tremendous struggle against American entry into World War II is related to his deep antipathy to imperialism and his bruising experiences with intellectual repression during World War I.

What I miss is not immediacy but just the reverse: I miss the perspective of distance. In leading us patiently through the complexities of Beard's evolving experience, Nore often leaves one uncertain about the larger contours of his thought. In view of a certain condescension in Beard's early outlook and his lifelong disinclination to confront the gulf in American society between white and black, how did he develop an unpretentious identification with, and confidence in, ordinary people? How can we account for Beard's striking (though never very successful) shift, in the 1920s and after, from economic interpretations to an emphasis on the role of ideas in history? What generated the passion and urgency of

his campaign for historical relativism? What explains the fierce nationalism of his later years? How were these deep-running changes interconnected? Nore gives us scattered clues and a new incentive to press such questions.

JOHN HIGHAM
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JOHN C. BURNHAM. *Jelliffe: American Psychoanalyst and Physician; His Correspondence with Sigmund Freud and C. G. Jung*. Edited by WILLIAM MCGUIRE. Foreword by ARCANGELO R. T. D'AMORE. Chicago: University of Chicago Press. 1983. Pp. xx, 324. \$20.00.

John C. Burnham has done much to illuminate the spread of psychoanalysis in the United States—his *Psychoanalysis and American Medicine, 1894–1918: Medicine, Science, and Culture* (1967) is probably the best discussion of the subject—and in this new book he focuses on a career especially important for this diffusion. Smith Ely Jelliffe studied Freudian ideas as early as 1905 and was one of the world's first few dozen psychoanalysts, practicing in New York by 1912. Originally attracted to Freud through Jung's work, Jelliffe retained for many years an eclectic interest in the ideas of both, although he gradually emerged as a fairly orthodox Freudian. He also developed a psychosomatic approach to disease and was among the first to investigate the psychological causes of physical illness. In addition, Jelliffe wrote several influential textbooks, and his career as a medical journalist—through the 1940s he owned and edited the *Journal of Nervous and Mental Diseases* and, with William Alanson White, superintendent of St. Elizabeth's Hospital in Washington, published the *Psychoanalytic Review*—was particularly influential.

Adopting Charles David Axelrod's concept (see *Studies in Intellectual Breakthrough*, 1979) of the "stranger"—an individual with a "defined relationship to the community but who still sees the world from a set of different perspectives" (p. xvi)—Burnham presents Jelliffe as one whose strong personality and journals brought before the American medical community the then-revolutionary ideas of Freud, Jung, and psychosomatics. Burnham sometimes provides a bit less context than many might need to understand fully the issues he discusses, but his approach well illustrates just what Jelliffe accomplished. The book is not a completely smooth biographical narrative but is always an insightful analysis of many topics: for example, the development of psychoanalysis in America, the rise of American medical specialization, the folklore of the American medical community, and, especially, the practice and importance of medical journalism in the early twentieth century.

William McGuire's edition of Jelliffe's correspondence with Freud and Jung supplements Burnham's biographical chapters effectively. The letters included here are drawn from the Jelliffe papers and the Freud papers at the Library of Congress and the Jung Archives. They are well edited, though McGuire, like Burnham, perhaps relies too heavily on secondary literature to provide the context for the documents he presents. This material allows one to see that, though Jelliffe saw Freud as his mentor, Freud probably never saw Jelliffe as one of his protégés. Despite various problems in the physical format of the edition—at times, two or three letters, followed by the notes to *all* of them, appear on one page, tending to confuse the reader—these documents will be valuable for the historians interested in issues beyond Jelliffe's career.

The volume usefully includes Jelliffe's full bibliography (compiled by John E. Sauer) and several interesting illustrations. In all, it adds much to our knowledge of the development of American medicine and psychoanalysis and provides a base on which further work on these topics can rest.

MICHAEL M. SOKAL
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WILLIAM H. BECKER. *The Dynamics of Business-Government Relations: Industry and Exports, 1893–1921*. Chicago: University of Chicago Press. 1982. Pp. xvi, 240.

Many United States diplomatic historians realize that their field faces a crisis of sorts. Existing approaches are not yielding compelling results. Foreign and domestic events refuse to remain separate, in spite of scholars' attempts to segregate them. Government records do not tell the whole story. At times, they do not tell even the most important story. Fortunately, William H. Becker moves beyond the normal range of analysis as he discusses business-government relations and the expansion of manufactured exports.

Two aspects of Becker's approach are particularly fruitful and allow him to demonstrate that revisionist historians have erred in arguing that businessmen became dependent on government services. First, he describes how structural changes in the international and domestic economies stimulated exports. The rapid development of industry in Western Europe and Canada in the early twentieth century created a growing demand for American goods. More important, following the 1893 depression, United States industry underwent a significant transformation. Firms became larger and established new labor control, production, and distribution techniques. These changes gave corporations great economic power in foreign markets. By 1914, major firms in key industries accounted for nearly

90 percent of manufactured exports. Able to succeed on their own, leaders of large corporations seldom wanted government aid, especially since it might trigger anti-American sentiments or open the door for domestic controls. Only the less significant smaller manufacturers solicited government support.

Second, the author indicates that business-government relations were fluid. A growing awareness of the need to influence policy on foreign branch banking, the merchant marine, and antitrust legislation effecting export combinations forced large industrialists to reassess their earlier stance. From 1914 to 1917, big businessmen and financiers began to cooperate with the Woodrow Wilson administration, principally through Secretary of Commerce William Redfield. Although reasonably successful in achieving their goals, business leaders continued to harbor deep-seated fears about government intrusion into their affairs. The wartime experience heightened these concerns, and, by 1921, spokesmen from the world of big business and finance championed a reduced role for the federal government in economic affairs, a leading characteristic of the ill-fated New Era.

Not all aspects of the author's approach are as persuasive. His use of a variation of the bureaucratic politics thesis is not very illuminating. The most significant shortcomings occur because he does not broaden his scope of analysis sufficiently. Becker seems reluctant to situate large corporations in their proper sociopolitical context. As David Gordon, Richard Edwards, and Michael Reich have shown, the structural changes in American industry were not merely a response to impersonal market forces. Corporate leaders forged an entire social system that vastly privileged capital over labor and gave corporations considerable independence for a few years. This new social structure of capital accumulation helped shape business-government relations. Becker also ignores the existence of a liberal-labor-left challenge to corporate dominance that peaked in World War I. Worried about these forces, business leaders and conservatives responded with the New Era.

Therefore, the reasons for the dynamic nature of business-government relations are more complicated than Becker suggests. Diplomatic historians must discover how the internal distribution of power among classes influences their subject matter. Although unwilling to go this far, Becker has taken a step in the correct direction.

FREDERICK C. ADAMS
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KIM MCQUAID. *Big Business and Presidential Power: From FDR to Reagan*. New York: William Morrow. 1982. Pp. 383. \$17.50.

Big Business and Presidential Power will not satisfy those expecting a comprehensive treatment of post-1932 relations between business and government, big business politics, or presidential power brokering. But, despite the need for a more descriptive title, Kim McQuaid has written an important book illuminating a significant but previously obscure aspect of business-government interaction. From it one can learn much about the efforts of America's business leaders to build institutions through which they could direct the growth of federal power along lines serving the collective interests of the corporations they headed.

McQuaid begins with the New Deal, focusing particularly on the establishment of the Commerce Department's Business Council, its failure to develop institutional foundations for a "corporate self-regulatory order," and its subsequent search for a new basis of cooperation between corporate power and reformist government. In this self-selecting body of corporate executives, he argues, one has not only a "barometer of big-business thought and action" but also a new kind of agency, one that was "in the government but not of it." And, in the agency's experiences, he sees reflected both the New Deal's willingness to cooperate with corporate power and its capacity to resist corporate direction and to undertake initiatives that most corporate leaders viewed as socially disruptive and system threatening.

In the ensuing chapters, McQuaid carries the story through the war and the postwar periods, noting the wartime arrangements for using but "containing" corporate influence, the acceptance of an "activist state" reflected in the "planning" of the Committee for Economic Development (CED), the successful "containment" and "channeling" of that state during Truman's presidency, the celebration of a new collaborative capitalism during the Eisenhower era, and the alternating spats and reconciliations of the Kennedy-Johnson years. The CED, as he depicts it, was a creation of Business Council activists that proved much more successful than their creations of the 1930s. But, even in the 1950s, their ability to contain anticorporate impulses was limited. And, under the conditions of the late 1960s and early 1970s, the state again became "activist" and politics "disorganized" in ways that led corporate leaders to seek new instruments of containment. They forged, in particular, the Business Roundtable and were able through it to reestablish and enhance their "political wallop."

McQuaid's study is unlikely to be the last word on its subject. One could legitimately quarrel with his selection of incidents on which to focus, his failure to see the post-1932 story in longer perspective, and his apparent conviction that understanding such business activity provides a better explanation of policy output than does the conventional emphasis

on liberal-conservative interaction. The latter is not fully persuasive on the basis of the evidence offered. But he does show convincingly that big business activists are better viewed as corporate politicians than as an omnipotent elite or antistatist defenders of free markets. He deserves high marks for the diligence with which he has ferreted out information difficult to secure and for the literary skill with which he presents it. He provides numerous insights likely to be of value to future researchers.

ELLIS W. HAWLEY
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RUTH SCHWARTZ COWAN. *More Work for Mother: The Ironies of Household Technology from the Open Hearth to the Microwave*. New York: Basic. 1983. Pp. xiv, 257. \$17.95.

Ruth Schwartz Cowan has written about the history of household technology and housework in a book for general as well as scholarly readers. Her easy, informal prose and minimal footnoting will please the general audience, and historians will appreciate her thorough bibliographic essays. By most accounts, industrialization brought more leisure to the home, because work was either removed from home to factory or else reduced by new labor-saving household equipment. Cowan takes issue with this conventional wisdom.

Her discussion of the stove, one of the most notable advances in nineteenth-century household technology, exemplifies her approach. Stoves actually reduced the men's work of gathering, splitting, and hauling fuel, since stoves consumed less fuel than open fireplaces, and men and boys were thus freed for school or work. But because the stoves permitted several types of cooking to be carried on at once—boiling, simmering, and baking, all on one fire, for example—they probably increased women's work by discouraging one-pot meals and encouraging the preparation of several different dishes for each meal. Thus, Cowan shows how the technology of early industrialization helped to define housework as more exclusively women's work. But she is also led into a bolder contention: industrialization, far from reducing women's work in the home, actually created "more work for mother." Even when some female chores disappeared, "almost every one was replaced by other chores, equally time and energy-consuming" (p. 65).

This argument becomes less tenable when Cowan carries it into the twentieth century. She must concede that some work related to food, clothing, and health care has indeed tended to move out of the home. Nevertheless, she contends that the reduction of most labor has been partial at best, while the work of transportation has actually increased. Fewer de-

livery services are offered, and the automobile has converted households from consumers to producers of transportation services. The suburban mother's chores as chauffeur thus figure heavily in Cowan's argument. So too does the declining availability of domestic servants in the twentieth century, causing upper- and middle-class housewives to shoulder more work.

Cowan concludes that "modern labor-saving devices eliminated drudgery, not labor," and that modern technology increased the housewife's productivity dramatically (p. 100). Cowan thus seems to concede her own argument after all. If drudgery is eliminated, is not work eased or reduced in some sense? Does not increased productivity literally mean that less work would yield the same results?

By focusing on the quantity of housework—is it more or less?—Cowan neglects important questions of motivation and ideology. We need to understand better why women so often devoted the gains realized through household technology not to leisure but to more elaborate housekeeping. Discussing the failure of alternative arrangements for the performance of household work, from utopian communities and cooperatives to profit-making ventures such as cooked food delivery services, Cowan finally cites most people's devotion to values of "privacy and autonomy" (p. 149). This would have been a better way to begin the inquiry than to end it.

Sometimes, greater attention to economic forces would have been helpful. For example, Cowan wonders why nursing homes, though expensive, have flourished while day care, equally expensive, languishes. This is no mystery when we consider the massive public funding made available to nursing homes but not to child care. Emphasizing continuing inequities between men and women in the performance of housework, Cowan passes over the greater equity between classes implicit in the decline of domestic service.

The strength of Cowan's work is her consistent ability to demonstrate how tools have shaped human behavior. Although the argument summarized in the title is unconvincing, Cowan's book is knowledgeable, deft, and stimulating.

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HASIA R. DINER. *Erin's Daughters in America: Irish Immigrant Women in the Nineteenth Century*. (Johns Hopkins University Studies in Historical and Political Science, 101st Series, number 2.) Baltimore: Johns Hopkins University Press. 1983. Pp. xvi, 192.

After providing background information on nineteenth-century conditions in Ireland and patterns of

female migration, Hasia R. Diner's keenly explorative study details the adaptations made by Irish women to the social and economic challenges presented to them during a period of migration that is seen as one of "incredible continuity." The struggle for subsistence in Ireland and America; the changes in family roles due to a rural to urban transition; the failures, breakdowns, and resulting pathology; and the female systems of support that evolved to sustain Irish-American families—all are acutely documented. A final chapter tries to strike a balance in judging relations between the sexes in Irish-American life in the nineteenth century.

This evaluation of female responses to social collapse in Ireland and the harsh realities of immigrant survival in America makes skillful use of government sources of information, the views of reformers and those close to Irish-American families, biographical material, and previous works on the Irish that are now seen from the perspective of women's history. What emerges is both a forlorn and an inspiring picture. Irish women are credited with having developed a family system unique among European peoples, a family pattern in which they played a very strong role and achieved economic benefits for their hard-pressed children and relatives. It was a family formula, however, that required either the foregoing of marriage or delayed marriage, yet very high fertility in marriage. After migration the role of the husband and father diminished, and greater influence accrued to females. This rigid, sacrificial, distinctly unromantic, conubial formula is traced by the author to the Great Famine of the 1840s that had such profoundly morbid effects on Irish life.

The attribution of the distortions in normal family behavior among the Irish to the famine can be explained in part by the land system in Ireland and the necessity of emigration, but an even broader view would take into account the enforced poverty and relentless degradation fastened on a subject people by pernicious English rule in an age of rampant imperial power. The act of emigration itself, with its own shoals of misfortunes, also brought delays in adjustment, marital decisions, and the assurances needed for rational family establishment. The exaction of savings needed for remittance to Ireland to rescue others and to stave off destitution was in itself a potent deterrent to marital plans. The famine was only one wave of tragedy for a people struck by successive waves in the last century.

The author states: "Irish-American culture de-emphasized romance. Social realities created a pattern whereby marriage and the interaction between husband and wife was at best one of irritability and separate spheres and at worst one of tension and domestic violence." Certainly the pressure of super-

rationalist Catholic family strictures and ideals and the Victorian segregation of sexes created tension, but an acquaintance with folk patterns of traditionalist behavior would argue for a more subtle and less severe interpretation. The participation of Irish women in nationalist endeavors is underestimated also, for the Ladies' Land League and later revolutionary activities such as that described in Margaret Ward's *Unmanageable Revolutionaries* provides a different picture. The explanation of the class and cultural distance that estranged Irish women from early feminist efforts is well stated. This work is a richly researched and penetrating inquiry in women's history that moves beyond the now frequent cataloging of mainstream feminists and pioneer mothers. The portrait of joyless and constrained sex roles and marriages needs correction from other sources that confirm the more flexible and rollicking strains in the Irish personality and disposition. The derivation of peculiar Irish family patterns from the famine must be complemented with evidence comprehending the exploitation surrounding the Irish from other quarters both in Ireland and America.

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BERNARD MARINBACH. *Galveston: Ellis Island of the West*. (SUNY Series in Modern Jewish History.) Albany: State University of New York Press. 1983. Pp. xx, 240.

The history of the great migration of East European Jews to America has been examined by scholars in American history and American Jewish history from many vantage points. But one chapter in that colorful saga, the Galveston movement, has not received the scholarly attention it deserves. Bernard Marinbach's thoroughly researched and carefully written book fills that lacuna. The story of the Galveston project is told chronologically with the focus shifting back and forth from the efforts of the sponsors and directors to the experiences of the immigrants. The author skillfully examines the complex interaction of ideology, politics, human need, and the twists of circumstance involved in the recruitment of about ten thousand Jews in Eastern Europe between the years 1907 and 1914, their transport to Galveston, and their resettlement in the American West.

The preeminent sponsor of the Galveston movement was Jacob H. Schiff of New York. Schiff was to American Jewry of the twentieth century what Moses Montefiore was to British Jewry in the nineteenth century: philanthropist, spokesman par excellence for Jewish causes, and defender of Jewish

rights. Schiff saw the Galveston project as a means of deflecting congressional enactment of restrictive immigration by demonstrating that Jewish immigrants were not congesting the metropolitan centers of the eastern seaboard. This goal was never realized but, as Marinbach points out, Schiff's confrontation with U.S. immigration officials and his escalation of political pressure on the Taft administration provided the paradigm for a new militant political approach for American Jewish leaders.

In Europe the Galveston movement was championed by Israel Zangwill, the celebrated Anglo-Jewish author and president of the London-based Jewish Territorial Organization (ITO). Zangwill and his associates were disappointed Herzlian Zionists who saw in the Galveston project a temporary substitute for their ideal of an autonomous, legally recognized, Jewish territory anywhere in the world. Rebuffed by the Zionists over Uganda, ITO was an organization looking for a *raison d'être*, which Schiff's project provided. Thus, the Galveston movement reflected in microcosm the great Jewish ideological debates of the early twentieth century.

The Galveston movement brought to the fore a talented group of Jewish social workers who created an organizational network spanning the European continent and reaching into the cities and towns of the American West. These men laid the foundations for the international Jewish relief organizations and the federation of American Jewish philanthropies. This volume illuminates the activities of one of these social workers, Jacob Billikopf, who made Kansas City, Missouri, a model for the resettlement of Galveston immigrants. I found the information dealing with Billikopf a significant adjunct to my own work, *Mid America's Promise: A Profile of Kansas City Jewry*, and to Frank Adler's *Roots in a Moving Stream*. Marinbach's study is an important complement to Jewish communal histories in the West and Midwest.

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DIRK BAVENDAMM. *Roosevelts Weg zum Krieg: Amerikanische Politik, 1914–1939*. Munich: Herbig, 1983. Pp. 639. DM 48.

This is a frustrating book to read. It is obvious that Dirk Bavendamm (a free-lance writer with a Ph.D. in history) has taken the time to master most of the literature concerning Anglo-American relations and the origins of World War II. He has even done extensive research in the Public Record Office. After summarizing American diplomacy and Franklin Delano Roosevelt's foreign policy views from World War I through 1936, Bavendamm provides a de-

tailed analysis of Anglo-American diplomacy from the president's "quarantine" speech in October 1937 to the outbreak of war in September 1939.

Most of his narrative will sound familiar to anyone who has read the recent literature by Hans-Jürgen Schröder, Bernd-Jürgen Wendt, Callum A. MacDonald, and David Reynolds, because Bavendamm retells what Reynolds has called the history of Anglo-American competitive cooperation. In particular, he emphasizes Neville Chamberlain's reluctance to give up his appeasement efforts, even after March 1939, since war would inevitably weaken the British empire and strengthen America's open door economic imperium. If this were Bavendamm's central thesis, his book could be welcomed as a well-written synthesis of recent scholarship for the benefit of the general reader in Germany. Unfortunately, this is not the case. Bavendamm makes a quantum jump to assert a controversial thesis that cannot be sustained by the evidence he offers.

According to Bavendamm, it was Roosevelt, not Hitler, who "dominated international events" (pp. 10–11) in 1938–39. It was FDR who viewed war as unavoidable after 1937 and therefore engaged in a "ruthless campaign" (p. 170) to sabotage Chamberlain's chances for an agreement with Hitler. Bavendamm does not deny Hitler's "tendencies toward war" (*Kriegsgeist*), but he insists that Hitler was neither the "monster" nor the "superman" depicted by most historians. It was Roosevelt who "incited" Hitler's war tendencies and provoked him to move prematurely and risk a major war. Had it not been for Roosevelt, says Bavendamm, Chamberlain might have reached a major settlement with Hitler that would have avoided general war in Europe (pp. 273–74).

When Bavendamm's views clash with the well-documented interpretations of other historians, he simply ignores them. He cites Boris Celovsky's book on the Munich conference but ignores Gerhard Weinberg's indictment of *The Foreign Policy of Hitler's Germany*. He does refer to the leading studies of FDR and Anglo-American relations, but he gives no inkling of the many instances of cooperation as well as competition between FDR and Chamberlain described by MacDonald, Robert Dallek, and others.

Even if you ignore Bavendamm's sins of omission, his argument is still unconvincing. He correctly notes that the critical months in the drift toward war occurred during the winter of 1938–39 when the British position hardened against Hitler's Germany. For Bavendamm, however, it was false rumors about Nazi plans for an attack on Holland and for an aerial blitz against London that turned Chamberlain against Hitler. And these rumors are blamed, without documentation, on FDR as well as on the German resistance (pp. 16, 385, 433–34). Bavendamm has confused cause with symptom. The crit-

ical point, as Weinberg has shown, is that Hitler's actions after Munich were "exactly opposite from what he had indicated to Chamberlain" (vol. 2: 464, 525–26). It was Hitler's actions against Czechoslovakia, not rumors of what he might do or any "ruthless campaign" by President Roosevelt, that brought Chamberlain reluctantly to the conclusion that any further move by Hitler must be opposed. Thus, the sequence of commitments to Holland, Poland, Romania, and so forth was symptomatic of Chamberlain's resolve to resist Hitler's efforts to dominate Europe by force. It was Hitler, not Roosevelt, who made war unavoidable.

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MANFRED JONAS. *The United States and Germany: A Diplomatic History*. Ithaca: Cornell University Press. 1984. Pp. 335. \$29.50.

Twice during this century the United States and Germany have waged war against one another and then have collaborated to create a peaceful, liberal-bourgeois world order. Appropriately, during the past fifteen years the stream of books about American-German relations, combined with the "new" international histories of postwar European reconstruction, virtually suggest that relations between these two nations have affected the modern international order more profoundly than bilateral relations between any other two countries. Therefore, this reviewer hoped that this survey by Manfred Jonas—author of an excellent book on American isolationism—would provide promised insights into the continuities and dynamics of American-German relations during the last one hundred years, or else a "state-of-the-art" narrative based on the rich and controversial literature. Unfortunately, *The United States and Germany* fails to offer the intellectual framework or penetrating analysis necessary for the scholar and lacks the narrative flair to engage the general reader.

Jonas usefully sketches the transformation of American-German relations from an initial era of "good feelings"—that apparently rested on the absence of imperial rivalry in Latin America and Asia and on American misperceptions about German society—to late-nineteenth-century hostile attitudes arising from conflicts over commerce, tariffs, ports in Samoa and the Philippines, and then Kaiser Wilhelm's admonition to his troops to give no quarter during the Boxer Rebellion. But precisely as the United States and Germany begin to affect one another and world politics more directly and profoundly—from the First World War to the present—Jonas's book becomes less engaging as he

inclines toward safe, but perhaps dated, positions, skirts significant issues—including considerations about third countries and neoimperial rivalries—and treats later major developments superficially.

Jonas's view that it was inevitable that the United States would side with Great Britain over Germany in 1917 is sound, although he exaggerates the import of the House-Grey memorandum, minimizes Germany's growing imperial war aims (which perhaps increased its determination to preclude American mediation), and does not differentiate Germany's capacity from its willingness to pay reparations, whose political and economic significance for the French are hardly noted. Jonas does not fully address the extent to which American statesmen viewed German reconstruction and reintegration as central to their goals, whether "containing" France, the Soviet Union, and social upheaval or the more ambiguous aspects of Gustav Stresemann's fulfillment policy or the contentious diplomacy in 1930–32 of Heinrich Brüning.

Developments in the Nazi era seem devoid of their high tragedy, while surprisingly little is said about the world views and diplomacy of Franklin Roosevelt and Adolf Hitler, the significance of differing American and British appeasement policies, or whether politics or economics determined the ultimate American-German confrontation. Jonas's opinion that Roosevelt's attitude toward fascism precluded meaningful relations after 1937 ignores too many diplomatic overtures, while belief that Germany showed "remarkable restraint" even in 1941 minimizes Hitler's effort to contain the United States by encouraging Japanese advances.

Jonas assesses how the constraints of wartime diplomacy and of interdepartmental debates promoted ambiguous planning for postwar Germany, but he misleadingly understates Roosevelt's commitment at Yalta to approximately \$20 billion in German reparations and ignores the Truman administration's reversal of that decision—for better or for worse—in favor of limited zonal reparations, which was less a "compromise" than a "take-it-or-leave-it" proposal at Potsdam with enormous implications. Finally, Jonas's telescoping of ensuing developments—the Marshall Plan, Berlin crises, integration of two rival Germanies into rival alliances, onset of détente—suggests a Cold War primer rather than scholarly explication, and he does not analyze German leadership or initiatives from Konrad Adenauer through Willy Brandt. Jonas's assessment of America's post-1945 German policy thus neither illuminates the origins or evolution of the Cold War conundrum nor provides for comparison with past eras. Consequently, his conclusion that the United States has usually supported a democratic, united, and economically viable Germany is scarcely revealing either about American

foreign policy or the dynamics of a relationship that has been immensely significant, past and present, for both countries and world politics.

ARNOLD A. OFFNER
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JUSTUS D. DOENECKE. *When the Wicked Rise: American Opinion Makers and the Manchurian Crisis of 1931-1933*. Cranbury, N.J.: Associated University Presses, for Bucknell University Press, Lewisburg, Pa. 1984. Pp. 188. \$24.50.

This compact, somewhat narrowly focused study of American elite opinion tells almost everything one might wish to know about public reaction to the Japanese conquest of Manchuria. Justus D. Doenecke's purpose is not to explain the causes or direct consequences of this event or even where it fits into the pattern of interwar conflict and diplomacy. Although obviously well versed in the existing literature, he has sought to fill a remaining gap, one dealing with the influence, or lack thereof, of public opinion on policy makers. By examining the public and private statements of numerous peace groups, religious organizations, business associations, journalists, and individuals, the author reconstructs the positions of those elite opinion makers who were best informed and most concerned about the Manchurian crisis.

Generally, Doenecke confirms what most historians probably suspected. The large majority of opinion makers bitterly opposed Japan's military expansion in 1931-32 because it threatened to shatter the fragile peace structures created at Versailles and during the Washington Conference of 1921-22. Concern for China was, at best, a secondary factor. Opinion makers tended to support efforts by Secretary of State Henry L. Stimson to apply political pressure and moral suasion against Tokyo. They approved the decision by the Hoover administration to permit tentative cooperation with the League of Nations and agreed with Stimson's "non-recognition" doctrine of 1932. On the thornier question of imposing either military or economic sanctions, however, the consensus among elites fell apart. Although a few favored more assertive moves, most preferred to avoid anything that risked provoking a confrontation with Japan. Nor did many elite representatives agree on the virtue of economic sanctions.

Doenecke suggests that the Hoover administration mirrored these opinions, even if the so-called opinion makers had limited access to policy makers. Hoover and Stimson supported political pressure against Japan and the effort to chastise its behavior but shied away from any use of force that might elicit a strong reaction. Curiously, the author does

not really make clear whether he believes that the elite opinion makers had much influence on foreign policy. In any case, the reader interested in learning how important groups viewed Japanese aggression in the 1930s will find this a well-researched and useful summary.

MICHAEL SCHALLER
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JOHN J. SBREGA. *Anglo-American Relations and Colonialism in East Asia, 1941-1945*. (Modern American History.) New York: Garland. 1983. Pp. xiii, 332. \$29.00.

"This is not virgin territory," concedes Warren F. Kimball in his generous introduction to this book, "far from it." Indeed. The superb studies by Christopher Thorne on Anglo-American wartime relations in the Pacific and William Roger Louis on the problem of colonialism in Anglo-American relations, both published in 1978, explored those intertwined topics in exhaustive detail. The specialist can hardly glance at John J. Sbrega's title without wondering if he has found something new to say. For the most part, despite an impressive amount of research in British and American archives, he has not. Other than supplying some fresh quotes and uncovering some useful new documents, Sbrega adds little to a story that is by now familiar.

The absence both of originality and thematic unity seriously limit the study's usefulness. Eschewing the standard chronological approach, Sbrega treats a series of related but discreet topics. A mere listing of some of these suggests their familiarity to the legions interested in this much-studied subject: the Atlantic Charter, the relationship between Franklin D. Roosevelt and Winston Churchill, trusteeship, the problem of French Indochina, and postwar planning. Other choices appear more idiosyncratic. The author provides a lengthy examination of the Pearl Harbor investigation and debate, a subject that is only marginally related to his main focus. Similar digressions on matters such as differences between Washington and London on military strategy appear periodically throughout the text.

These are not the only major weaknesses of the book. Equally problematic are some of the author's interpretive judgments. His continuous references to what he variously describes as the "genuine praiseworthy motives," the "good intentions and high ideals," and "the laudable goals" of the United States prove not only simplistic but surprisingly uninformed by the rich scholarship on the complex question of American colonial policy. Sbrega's failure to take account of the more sophisticated arguments advanced by Thorne, Louis, and others on this subject is simply inexplicable. Convinced that

American objectives in the Pacific were both idealistic and reasonable, he deplores FDR's failure to secure those objectives. Sbrega labels the result a "tragedy," attributable in his view to the president's chronic inability to coordinate means and ends in his diplomatic dealings. Yet the author's own evidence suggests the plausibility of an alternative explanation: that Roosevelt's failure to translate his anticolonial sentiments into effective policy resulted from his quite realistic recognition during the closing years of the war that the need to maintain harmony within the Western alliance was an overriding concern.

This volume is clearly the product of a prodigious researcher. The new material unearthed by the author constitutes the book's principal strength; regrettably, his failure to integrate that material into a fresh or convincing interpretation proves a fatal drawback.

ROBERT J. MCMAHON
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DOROTHY M. BROWN. *Mabel Walker Willebrandt: A Study of Power, Loyalty, and Law*. Knoxville: University of Tennessee Press. 1984. Pp. xvii, 328. Cloth \$29.95, paper \$14.95.

Mabel Walker Willebrandt rose from simple beginnings in rural Kansas to glittering prominence as an assistant attorney general in the Justice Department of the 1920s. Caught between contending forces within the Republican party during the election of 1928 and undermined by her efforts to enforce the Eighteenth Amendment, Willebrandt left government service for a pioneering practice that encompassed aviation and radio law. Despite a network of powerful Washington allies and her abiding loyalty to the Republican party, the federal judgeship that she sought eluded her. Instead, the former champion of prohibition enforcement went on to represent the California grapegrowers as well as Hollywood notables such as Louis B. Mayer. During the height of the second Red Scare and near the end of her career, she represented the Screen Directors Guild.

From this truncated profile, it should be evident that Willebrandt is a significant figure worthy of the extensive research that Dorothy M. Brown has devoted to her in this biography. Willebrandt is significant not so much because her career happened to converge with some of the major legal and political currents of her time but largely because she was one of a handful of women to thrive in the upper echelons of government and the legal profession. It is the matter of gender that transforms her moderately interesting career into an extraordinary one and provides the interpretive challenge for her

biography. As Brown puts it in the preface, Willebrandt's "life provides a case study for women's history as she met major challenges in identity and autonomy" (p. xiii).

Brown has ranged widely and dug deeply to assemble her portrait, especially for the post-1930 period when Willebrandt faded from public view. The portrait, however, is essentially a narrative account, abundant in detail and spare in analysis. Although Brown is astute at linking Willebrandt's career to legal and political developments, she fails the more difficult challenge of shaping the disparate and intriguing components of Willebrandt's life into a coherent whole. Most readers will find Brown less than successful in placing Willebrandt within the contours of women's history. If her life is a case study, Brown does not evaluate the import of this particular case.

Some, moreover, will take issue with Brown's treatment of Willebrandt and the House Un-American Activities Committee. It was Willebrandt who drafted the loyalty oath for the Screen Directors Guild. It is apparent that she was militantly anticommunist and eager to protect her clients, but it is not apparent to this reader that her stance reflected a concern for individual freedom, as Brown suggests. In two custody cases, Willebrandt used disloyalty against unfriendly HUAC witnesses, citing them as unfit mothers.

The great strength of this biography is in its richness of personal detail, much of which implicitly illuminates the tensions engendered by Willebrandt's launching and sustaining of an amazingly successful legal career. The book is handsomely mounted, studded with photographs and newspaper cartoons, and meticulously annotated. Flaws notwithstanding, it is a welcome addition to the literature of women's history because of its exhaustive accumulation of data on the life of a brilliant and ambitious female attorney.

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GARY DEAN BEST. *Herbert Hoover: The Postpresidential Years, 1933-1964*. Volume 1, 1933-1945; volume 2, 1946-1964. (Hoover Press Publication, number 275.) Stanford: Hoover Institution Press. 1983. Pp. xvi, 277; 280-522. \$75.00 the set.

These two volumes document in detail for the first time Herbert Hoover's major ideas and activities during his long postpresidential career from 1933 to 1964. Unfortunately, this valuable and needed study suffers from several liabilities. The first, I suspect, Gary Dean Best could not control; namely, the decision by the Hoover Institution Press to bring

the work out in two highly priced volumes. Presumably, a single 522-page book could have been marketed more reasonably. Moreover, the fact that the second volume contains all the footnotes, in addition to the index, deprives specialists of the periods before and after 1945 from simply purchasing one volume. An annotated bibliography accompanying each volume would have been much more useful than filling sixty-six pages with footnotes set in the same size type as the text.

Turning from marketing and formatting matters, another liability under which this work labors is the absence of a clear analytical or methodological approach to Hoover's thoughts and actions as an ex-president. Both are narrated in great detail, but often Best sacrifices analysis to description, instead of attempting a balance between the two. He also too readily attributes influence to Hoover without taking into account the enormous disadvantage ex-presidents face when they try to affect national policy. In fact, there is no theoretical discussion of the dilemma faced by all former occupants of the White House because as yet there is no institutionalized way for them to participate in the formulation of domestic and foreign policy. As a result, Best exhibits a tendency to exaggerate Hoover's public impact during most of the thirty-one years he occupied that most equivocal position in American political life—the office of the ex-president.

With the exception of his influence over famine relief immediately following World War II and Truman's and Eisenhower's governmental reorganization plans, Hoover's policy-making role as an ex-president was not unique for being minimal, especially in the area of his greatest expertise: foreign policy. Best reaffirms the established revisionist view of Hoover as an astute, if unpopular, critic of domestic and foreign affairs from 1933 until the mid-1950s. He does not, however, add much interpretative subtlety (simply more detail) to Hoover's revised image. For example, when discussing the *Challenge to Liberty*, published in the fall of 1934, Best simply cites figures for the number of copies printed, newspaper opinions, and a single secondary source to arrive at the conclusion that the book spearheaded a "businessmen's crusade against the New Deal" (p. 27). There is no attempt to explain why Hoover came to write a theoretically abstract criticism of the New Deal rather than a statistical one more in keeping with his engineering background and previous political career or to use sources showing its direct impact on the business community.

The same assumption of influence and lack of close content analysis characterizes the account of *Problems of a Lasting Peace*, a book Hoover wrote with retired diplomat Hugh Gibson in 1942. After detailing the numerous attempts Hoover and his friends made to promote this book, Best neglects to men-

tion that few of its most important and controversial suggestions were even given practical implementation by either Congress or the White House. Instead, he maintains that the book was directly responsible for the Mackinac declaration issued by the Republican Advisory Council in the fall of 1943, and he subsequently asserts that "the Hoover-Gibson program . . . had done more . . . than Willkie or any other individual to prepare the Republican party to accept American participation in a postwar international organization" (p. 240).

Throughout both volumes Best seldom distinguishes between Hoover's often murky rhetoric and the harsh reality he faced as ex-president, particularly as he tried to bring about a negotiated peace with the Japanese in the spring of 1945; his defeat in the "Great Debate" over early Cold War policies in 1950; and finally his own most pessimistic views of his influence over policy contained in a private memorandum written on September 4, 1954. John Foster Dulles's increasingly important position within Republican circles in the late 1940s, and especially after he became Eisenhower's secretary of state in 1953, explains much of Hoover's marginality in the formulation of postwar foreign policy. Yet Best concludes that Hoover's "policies . . . heavily influenced the American defense posture under Eisenhower" (p. 437).

This preoccupation with proving Hoover's influence, notwithstanding, these two volumes contain concrete proof that there is life after the presidency even when a president leaves office, as Hoover did, discredited by a major national trauma.

JOAN HOFF-WILSON
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ROBERT H. FERRELL. *Truman: A Centenary Remembrance*. New York: Viking or Penguin, Markham, Ontario. 1984. Pp. 256. \$25.00.

Truman buffs especially will appreciate this "centenary remembrance" of the thirty-third president. It is a collection of photographs—some shown here for the first time—and an extended essay on Harry S. Truman's life similar to that which Joseph Alsop did on Franklin D. Roosevelt. Almost half (112 out of 250) of the pages are devoted to photographs.

Robert H. Ferrell has made good use of the recently opened "Dear Bess" letters, which he edited, to give us a more thorough portrait of Harry the man than most biographies contain. Slightly over half of the text covers the prepresidential years. Coverage of the presidency stresses Truman's formulation of American foreign policy in the Cold War and scarcely touches on domestic affairs.

There are few surprises, although this reviewer was astonished to learn that farmer Truman moved

his fence rows twice a year "in order to keep them clear of weeds and waste" (p. 42) and that FDR told his running mate about the atomic bomb project in August 1944 (p. 110). There are endnotes, but, unfortunately, this latter bit of important information has no citation. Ferrell continues to believe, as in his previous works on Truman, that America was most fortunate to have this man in the White House during a crucial time in world history.

The writing is excellent and the interpretations conform largely to the standard consensus viewpoint. Occasionally the judgments are questionable, such as lumping James Byrnes, James Forrestal, and Robert Lovett together with George Marshall and Dean Acheson as "extraordinarily able appointments" (p. 188). Also, Robert Taft is viewed as having "had a genius for taking false positions. . . . He would have been a pushover for any Democratic candidate in 1952" (p. 221). But overall this is an interesting and attractive addition to the growing body of Trumaniana.

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MONTAGUE KERN *et al.* *The Kennedy Crises: The Press, the Presidency, and Foreign Policy*. Chapel Hill: University of North Carolina Press. 1983. Pp. xiii, 290. \$29.00.

This book analyzes "press-presidential interaction" and "day-to-day coverage in five important newspapers during four major crises . . . Laos and Berlin in 1961, Cuba in 1962, and Vietnam in 1963" (p. xi). Montague Kern and her associates insist on the "primacy" of newspapers during the Kennedy years, claiming, without evidence, that evening television news "offered almost no detailed analysis of foreign policy issues" (p. xi). The book offers "insight" about how the press can influence a president during a foreign policy crisis and vice versa. In terms of methodology, this book will interest students of communication more than historians, for here content analysis grows out of elaborate tabulations of data attempting to measure not a reporter's opinion but his sources: "What is significant about news coverage . . . is more where reporters obtained their material than how they felt about it" (p. 239). The authors modestly offer no conclusions about causation in describing Kennedy's relations with the press, though they do believe the press was "largely a reflective institution" (p. 197), because of the timing of issues, partisanship, and the influence of newspaper constituencies.

The Kennedy Crises is a deeply flawed book, in spite of much careful research. Imagine how disappointing it would be to read a thorough analysis of a great screen beauty that said nothing of her face. Here the missing ingredient is television. Chalmers Roberts

and I took the authors to task about ignoring television when they presented an earlier version of their research at a 1981 conference. The authors continue to insist that, because every evening television news broadcast for the period 1961-63 has not been kept, it is therefore impossible to say anything about how television shaped Kennedy's foreign policy making. The authors actually bring up television quite often—more than the index indicates. They have nevertheless chosen to ignore transcripts of television interviews with Kennedy, which are among the most-quoted parts of the *Pentagon Papers*. They manage to discuss the Berlin Wall without once mentioning the extraordinary televised impact of Kennedy's "Ich bin ein Berliner" speech, seen all over West Germany as well as in the United States. They have made almost no use of manuscript materials in the Kennedy Library relating to television news and seem unaware of the selected television newsfilm in the same library. The newspapers they cite so diligently are actually filled with comment about television news and Kennedy's constant use of television.

Students of foreign policy will find that this book, which claims to ignore television, actually contains a valuable discussion of Kennedy's Cuban missile television address of October 22, 1962, in particular a memorandum about the televised impact of that speech by Richard Neustadt (pp. 126-27). Ironically, the authors admit that Kennedy, discussing Vietnam in 1963, "use[d] television news programs to make most of his substantive comments on Vietnam" (p. 166). What Joseph Alsop said in a column did not have as much impact in 1963 as what television news commentators were saying. The authors have overlooked the chapter on Kennedy in James Pollard's *The Presidents and the Press: Truman to Johnson* (1964) that presents a helpful starting point. *The Kennedy Crises* contains valuable data for a more significant conclusion: the eclipse of newspapers by television as the president's central medium of communication. It is fortunate that other scholars are less timid about using less-than-complete data in studying television's impact on foreign policy making.

DAVID CULBERT
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RICHARD D. MAHONEY. *JFK: Ordeal in Africa*. New York: Oxford University Press. 1983. Pp. viii, 338. \$17.95.

Black Africa has long remained the "dark continent" for historians of American foreign relations. Although they have concentrated on U.S. actions in Europe, Asia, and Latin America, political scientists, journalists, and former governmental officials have dominated the analysis of American diplomacy in

Africa. Unfortunately, most of these studies lack the documentation and detachment demanded by professional historians. They are based largely on secondary sources and are generally highly partisan. Richard D. Mahoney's study of John Kennedy's diplomacy in the Congo, Ghana, and Angola brilliantly illustrates the benefits of primary evidence and historical objectivity for an account of American relations with Africa.

Mahoney, the son of Kennedy's ambassador to Ghana, has not only used the copious diplomatic records in the Kennedy Library, but he has also drawn information from over two hundred interviews with former U.S. and foreign officials. This impressive research adds abundant new detail to Kennedy's dealings with Africa and documents the deep splits within the foreign policy bureaucracy over the importance of the continent and the goals of U.S. diplomacy.

The author is also refreshingly objective in his evaluation of Kennedy. Mahoney recognizes the president's sympathy with African nationalism and his personal attention to African leaders but acknowledges Kennedy's mistakes, vacillation, and preoccupation with other issues.

Over half of the book is devoted to the continuing crisis in the Congo because this was the area of most sustained U.S. interest and most direct conflict among the major powers. Mahoney effectively unravels the seemingly endless factions, secessionist movements, and crises in the former Belgian colony and the often convoluted U.S. response. He judges Kennedy's support of the UN and of Congolese unity a success. Despite vocal criticism from African nationalists, European allies, and American conservatives, the president's restraint and persistence did not produce a major triumph but was generally effective. Mahoney is less censorious of Kennedy's actions than Stephen Wiessman's *American Foreign Policy in the Congo, 1960-1964* but is more critical than Madeline Kalb's *The Congo Cables*.

In shorter sections on American relations with Ghana and with Portugal over the independence of Angola, Mahoney finds less to praise. American funding of the Volta River dam in Ghana, despite strong misgivings about its mercurial leader Kwame Nkrumah, showed a willingness to accept African neutralism but did not end tensions between the two nations. The need to maintain an American military base in the Azores strongly eroded Kennedy's pressure on Portugal to move toward Angolan independence. Kennedy's efforts produced condemnation by both NATO allies and African leaders.

Mahoney's book is clearly written, tightly organized, and impressively documented. Its only weakness is a lack of recognition of the domestic influences on America's African policy. The civil rights movement and the growth of black political power

were major contributors to Kennedy's attention to Africa. Although this theme is not developed in the book, *JFK: Ordeal in Africa* is a major contribution to American diplomatic history and is a model of thoroughness and objectivity.

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ALLEN J. MATUSOW. *The Unraveling of America: A History of Liberalism in the 1960s*. (New American Nation Series.) New York: Harper and Row. 1984. Pp. xv, 542. \$22.95.

VAUGHN DAVIS BORNET. *The Presidency of Lyndon B. Johnson*. (American Presidency Series.) Lawrence: University Press of Kansas. 1983. Pp. xvi, 415. Cloth \$25.00, paper \$14.95.

These two books on a pivotal decade—the 1960s—differ in major ways but come close to agreement in their appraisals of the political leaders and the dominant ideology. They diverge in focus and point of view as well as literary qualities. One looks at the Johnson administration from the right; the other examines liberalism from the left. Neither expresses unmixed enthusiasm for its subject.

Vaughn Davis Bornet, author of works on welfare, labor politics, and Herbert Hoover, focuses on Lyndon Johnson's presidency. Part of the American Presidency Series published by the University Press of Kansas, Bornet's work is not a biography. Instead, it is a study of the official actions of the administration, both foreign and domestic. To appraise those actions, it ranges beyond the 1960s.

In contrast, Allen J. Matusow, author of studies on Truman's farm policies, Joe McCarthy, and the civil rights movement, emphasizes the history of liberalism in the Kennedy-Johnson years. His book, a volume in the New American Nation Series, is concerned chiefly with domestic affairs. Its central theme is the liberal's acquisition, use, and loss of power. Matusow deals with foreign matters only as they affected developments inside the United States but does more than Bornet with events outside of government, including the civil rights movement, the counterculture, the New Left, and black power. In his efforts to enlarge understanding, Matusow, too, moves beyond the 1960s, exploring both background to and consequences of developments of the decade.

The books, although different, do overlap significantly. Both Johnson and liberalism are major topics in each book. And for a brief period (1964-65) the president and the liberals joined in a rather surprising and unusually effective alliance, although one that fragmented later under the pressures of Vietnam.

Both authors depend heavily on published sources, primary as well as secondary, but also use some manuscripts. In his superior bibliographical essay, Bornet expresses a low opinion of many of the sources available to him, especially those in the Johnson Library, protesting that too much was closed there. After using both the Kennedy and Johnson libraries, Matusow writes more positively than Bornet of such institutions and of the oral history materials as well as the manuscripts available in them. He found the Kennedy more helpful than the Johnson and benefited from the fact that he did not have Bornet's need to explore the shaping of foreign policy, an area in which restrictions seriously hamper research in recent history.

In literary quality, Matusow's book is clearly superior. Bornet's paragraphs often lack unity; his book does not develop its suggestions or move smoothly from one paragraph and one topic to the next; and the material is not controlled by a clear, helpful organizing scheme or large, powerful themes and arguments. Matusow, in contrast, commands a superb historical style that is always clear, bold, and often exciting. His paragraphs and chapters are beautifully crafted; his overall organization is clearly defined. He engages the minds of his readers, seeks to affect their thinking, gives them ideas, yet does not overwhelm them with details and quotations. He impressively defines the character and significance of people and events and writes with striking confidence in his ability to discern the motives of his actors and to understand the theories of people like John Maynard Keynes and Milton Friedman.

Matusow's writing reflects a mind that perceives issues clearly and thinks in terms of development over time and of historical problems, their definitions and solutions. He repeatedly asks why human affairs moved along as they did in the 1960s. Why, for example, was the civil rights movement capable of putting great pressure on Kennedy? Why did the liberals move from advocacy of minor reforms to a "War on Poverty?" Why did the New Left, the counterculture, and black power flourish for a time and then quickly lose strength?

Perhaps the most important exception to Bornet's tendency not to think in terms of problems to be defined and solved is his treatment of Johnson's decision against running for reelection in 1968. Challenging other explanations and supplying substantial, persuasive evidence, he argues that poor health was the major explanation. Devoting much less space to this problem, Matusow sees health as a factor but reports evidence that suggests it was not the only one and emphasizes a political interpretation instead.

Bornet's angle of vision differs from Matusow's. Although both authors offer major criticisms of their subjects, Bornet is somewhat more positive. He

applauds many of Johnson's goals; he dislikes the strongly negative accounts, like Robert Caro's, that dwell on his personal traits. A biographer, he maintains, should approach his subject sympathetically; a student of a president should emphasize official acts and their consequences. Matusow finds Johnson's personal traits highly relevant to his official behavior.

Yet Bornet makes much of what he regards as major defects in Johnson's record. At home, he promised too much, especially when speaking of the Great Society and the War on Poverty. He also tried to do more than most Americans were willing to support and produced long-term problems, including inflation. In Vietnam, he failed to force a fundamental reassessment after he succeeded Kennedy, did not give the public an accurate picture, and did not use force properly. Although Bornet rejects the opinion that Johnson's programs, by generating unrealistic expectations, produced the upheavals of the late 1960s and admires Johnson's record in race relations, his critique resembles rather closely that of American conservatives of the past two decades. He writes that Barry Goldwater was "a zealot who was ahead of his time but clearly prepared the way for one who someday would come" (p. 115).

In a strange concluding chapter, Bornet attempts a final appraisal. It is obviously a difficult task for him, and the difficulty encourages him to go over and over his clashing opinions and to conclude with an emphasis on complexity: "The presidency of Lyndon B. Johnson will inevitably be remembered—and ought to be—for the characteristics of its central figure; for the unintentional but substantial damage that it did with some catastrophic policies abroad and erroneous policies at home; and, especially, for the many worthwhile changes it embedded deeply in legislation, in the lives of millions, and in American society" (p. 351). The emphasis on the last point does not seem compatible with the dominant spirit of the book.

Matusow comes down more clearly on the side of failure. He does see Johnson's contributions in civil rights as "the greatest achievements of his tenure," as finishing off "Jim Crow," and as cleansing "the poisoned atmosphere of southern politics" (pp. 180, 187, 188), but he also gives great weight to the limits of those contributions, especially their restriction to the South. More optimistic than Bornet in his view of the possibilities for change in human affairs, he sees the war in Vietnam, with its demands for resources, as a substantial limiting factor, a point on which he clearly disagrees with Bornet. But to blame the war is to embrace a liberal explanation for liberalism's failure to transform American life, and that is not this author's point of view.

For Matusow, the chief obstacle in the way of large-scale change was liberalism itself. Here the

crucial defect was not a tendency to promise too much but weaknesses in perception that rendered the liberals unable to deliver on promises, such as the assurances that liberal measures would create an orderly society. The liberals, according to the author's interpretation, were too soft. They assumed that significant changes come easily. They did not understand the problems they faced, including poverty, or the solutions to them, including the redistribution of income. Also, they did not think hard enough about the distribution of power and the need to change it. They were too willing to compromise with the corporations, the political bosses, and other power groups. In this account, the liberal failure and the rejection of liberalism in 1968 appear virtually inevitable.

The War on Poverty, Matusow insists, was "one of the greatest failures of twentieth century liberalism" (p. 220). The Vietnam War did much more than the poverty war to move people up the economic ladder, while no program of the period enlarged employment opportunities for the poorest people in the land.

Whereas Bornet appears to assume that change could not occur at the pace that Johnson and the liberals proposed, Matusow seems to believe that other leaders with a firmer grasp on realities and unhampered by war could have moved things forward at a more satisfactory pace. Yet these books are not likely to shake the faith of those who regard liberalism as the best instrument that American politics supplied during the 1960s for the improvement of American society. Bornet finds serious defects in Goldwater just as Matusow identifies faults of considerable magnitude in the people on the other end of the political spectrum. Perhaps the American political system was capable of promoting only small changes at the time and of being itself changed in but limited ways. Perhaps as well Johnson's expansive rhetoric was an essential part of the process, superior in its effectiveness (at least at his peak in 1964-65) if not in its insights to both conservative and revolutionary styles.

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CANADA

SYLVIA VAN KIRK. *Many Tender Ties: Women in Fur-Trade Society, 1670-1870*. Reprint. Norman: University of Oklahoma Press. 1983. Pp. 301. Cloth \$21.50, paper \$9.95.

Many Tender Ties is a pioneering work in Canadian women's and family history of the fur-trade era. Sylvia Van Kirk considers the shifting fortunes of three groups of women: Indian women who became

wives of the early generations of Euro-Canadian traders; mixed-blood (a historical term) women, descendants of the interracial unions, who by the late eighteenth century in central Canada had become the preferred partners of most of these men; and Euro-Canadian women who became increasingly important after 1821.

In addition to describing the various domestic roles that one would expect of these women, Van Kirk points out that, in the early years of the trade in most regions, Indian spouses through their kinship ties provided their husbands with access to native trading networks. In traditional native societies, kinship connections served to define the nature of socioeconomic relations both within and between bands. Also, in the early years, these women possessed survival skills that were very important to their husbands when traveling.

By the end of the eighteenth century, patterns of economic intercourse became well established, and marriage bonds with Indian women lost much of their economic significance. Consequently, Euro-Canadian men turned increasingly for their marriage partners to those local mixed-blood populations that were developing around the trading posts. Mixed-blood women therefore came to occupy the upper echelons of the emerging "fur trade society." But, their favored position was short-lived.

Of considerable importance is Van Kirk's demonstration that the relationships between native women and Euro-Canadian men were, in most instances, not ephemeral. Indeed, the notion developed of "country" marriages, or marriage "à la façon du pays." These were, when viewed from a Euro-Canadian perspective, in effect simply common-law marriages, and many lasted until one of the partners died. One problem of these unions was, however, that some traders spent their retirement years back in their mother country or in eastern Canada, where their native families would either not be welcome or would find it difficult to make a satisfactory adjustment.

Van Kirk's study provides a very readable account of an aspect of frontier life that has to date not received adequate attention. Her work, along with Jennifer Brown's *Strangers in Blood*, has served to widen the scope of research in this important dimension of Canadian history. Yet Van Kirk's work also raises more questions than it answers. Probably the most difficult one is, where do we go from here? It is clear from Van Kirk's and Brown's work that studies of women's and family history during the fur-trade era will severely test the mettle of the researcher. Information provided by women involved in the fur trade is extremely limited. Most of what is known has been obtained from the records of a very few prominent fur-trade families, most of whom lived in the Red River colony of historic

Rupert's Land. To go beyond Van Kirk's findings it will be necessary to abandon the narrative impressionistic style of her study and adopt a more rigorous social science–history methodology.

By broadening the data base and scope of the research, it will be possible to address other important issues. For instance, was there only one "custom of the country," or were there several? Local Indian marriage practices varied considerably. Did these different native traditions influence the character of interracial unions in the various locales? Were there also regional variants of "fur-trade society"? Indeed, one of the methodological problems of Van Kirk's study is that the term "fur-trade society" is never clearly defined. The concept of "community" is not explored either. More specifically, did all fur-trading communities look to the Red River colony as their model? Were Indian women in all areas eager to take Euro-Canadian men as husbands? Given that the quality of domestic life varied considerably among native groups, it would be surprising if the lure of life at a trading post was equally strong to all native women.

Clearly, the study of women's and family history on the fur-trade frontier is a very important new area of research. It will be very challenging and will require the development of sophisticated methodologies to make the most of records that yield relatively little direct information on the subject.

ARTHUR J. RAY
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FREDERICK W. GIBSON. *Queen's University. Volume 2, 1917–1961: To Serve and Yet Be Free*. Kingston: McGill-Queen's University Press. 1983. Pp. xvii, 518. \$49.50.

Ontario's universities have been studied with increasing frequency during the last decade. C. M. Johnston's study of McMaster, John Gwynne-Timothy's centenary volume on the University of Western Ontario, and Hilda Neatby's examination of Queen's are examples that come readily to mind. At their best, such books add a great deal to social and intellectual history; at their worst, they merely catalogue long lists of names and dates that have meaning only for alumni. Happily, this second volume of the Queen's official history represents the first category. Commissioned by the university to write its official history in 1969, Neatby died in 1975 before the project could be completed. Two members of the university's history department, Roger Graham and Frederick W. Gibson, completed and published volume 1, covering the years 1841–1917, in 1978. Gibson now has brought the study to the mid-1960s in this second volume.

The issues faced by Queen's in the first sixty years of the twentieth century have a decidedly contemporary ring. Lack of funds for major research, inadequate faculty salaries, and little or no capital for new buildings marked an essentially bleak financial landscape, made even more barren by depression and war. Private sources of funds were few while government sources were unreliable. Not until the late 1940s, for example, did the Ontario provincial government's grants match their previous high of 1929. The 1950s, marked by government surpluses and by a positive public view of higher education, saw an easing of these constraints and the emergence of the modern university.

Queen's also faced a series of decisions about the nature of its curriculum and the place of research among its priorities. Most private benefactors, such as R. S. McLaughlin, founder of General Motors of Canada, were attracted to the practicality of engineering and the sciences. Belief in the primacy of these disciplines was shared by some of the university's administrators, resulting in a long and steady decline in the humanities that began after World War I and only ended with the amazing prosperity that followed World War II. Financial constraints restricted serious scientific research, and in any case Queen's regarded itself only as a teaching institution. Few of its supporters and teachers saw a close and necessary relationship between the two activities. The result of all this was low faculty morale and academic mediocrity.

The university faced such problems and in the end overcame them because of the qualities of its leaders. Principal Robert Bruce Taylor (1917–30) lacked both a clear vision of the university's role and the managerial skills necessary to run the university efficiently and humanely. Rising discontent among faculty, students, and trustees finally forced him to resign. His successor, William Hamilton Fyfe (1930–36), was a Scot who was not at home in colonial Kingston. Not until the principalships of R. C. Wallace (1936–51) and W. A. Mackintosh (1951–61) was Queen's able to achieve the unity and purpose it had enjoyed under G. M. Grant in the late nineteenth century. Wallace realized that fiscal considerations had to take second place to academic ones, something the university's long-time treasurer, W. E. McNeill, would not accept. Mackintosh saw the pursuit of excellence as the essence of the university; for him, it was a community of scholars devoted to the best in teaching and research. Queen's modern reputation is largely based on the work of these two men and their three like-minded successors, principals J. A. Corry (1961–68), J. J. Deutsch (1968–76), and R. L. Watts.

This affectionately objective institutional history seems at times to be excessively concerned with trustees and administrators. Faculty occupy second-

ary positions, and students seem even more remote. That is perhaps as much a comment on the nature of the modern university as it is on Gibson's candid narrative. This welcome book has much to say about the role of the modern university community in the society that surrounds and supports it.

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LATIN AMERICA

JOHN M. KIRK. *José Martí: Mentor of the Cuban Nation*. (University of South Florida Book.) Tampa: University Presses of Florida. 1983. Pp. xi, 201. \$17.95.

"Travelers in Cuba are almost forcibly made aware of the overpowering presence of Martí throughout the Republic," observes John M. Kirk in his able and readable book. "In Havana, for example, they arrive at the national airport outside Havana known as the Aeropuerto José Martí; they may visit the imposing National Library, the Biblioteca Nacional José Martí, and the central square of Havana known as the Plaza José Martí."

Yet Martí was something of a late bloomer in Cuban thought, as Kirk, who teaches Spanish at Dalhousie University in Halifax, Nova Scotia, suggests. Not until the mid-1930s, some forty years after this Cuban patriot's death in 1895, did Cubans display an active interest in him. Even then, the view of Martí was, at best, somewhat limited. Cubans held him in awe. He was often compared with United States presidents ranging from Lincoln to Franklin Roosevelt and including even Wilson and Theodore Roosevelt. They went so far as to draw parallels between Martí and Jesus Christ! But they paid precious little attention to his ideas. He was, as Kirk notes, "one of the most underrated political thinkers of modern times."

That situation has changed dramatically in the twenty-five years since Fidel Castro came to power. Martí is now "presented as the underlying inspiration" of the Cuban revolution—the *autor intelectual*, to use Castro's own term. One of the revolution's main efforts in its early years was to focus attention on Martí's ideas "to demonstrate the direct relevance of Martí's teaching to the contemporary scene." Heavy emphasis was placed on Martí's numerous critical references to the United States. That, in turn, led to a campaign by U.S.-based Cuban exiles to demonstrate that Martí actually was decidedly pro-U.S.

The resulting debate, which Kirk ably sketches, was more shrill than intellectually balanced. Both sides went to extreme lengths to prove points. But out of this debate came, by the late 1960s, a reasoned, rational approach to Martí on the part of

Cubans on the island. A fresh generation of *martianos* (now headed by Roberto Fernández Retamar, a former professor at Yale) "concentrated on indicating and explaining what they . . . saw as the fundamental, if long ignored, substance of Martí's work—his political and social thought." Moreover, there has been a more reasonable, objective interpretation of Martí's views on the U.S. The main thrust has been to suggest a steady progression in Martí's thought from liberalism to anticolonialism and eventually to anti-imperialism. "The order of the day," writes Kirk, "is to present Martí as a convinced anti-imperialist, a man also with a profound interest in the situation of the working class, and of course a dedicated revolutionary." Gone are idealized and semimystical approaches to Martí; gone, too, are comparisons with Lincoln and Roosevelt. In their place is a much more understandable human being.

It is obvious that Kirk finds much to agree with in the new interpretations coming from Havana. "In actual fact," he writes, "a thorough study of his *Obras completas* reveals Martí to be a coherent thinker with clear and most definite priorities for the complete reshaping of Cuba after independence had been won from Spain." Would Martí have favored the Castro revolution? That is hard to say. But Kirk holds that Martí's writings "provide ample proof that he possessed a comprehensive blueprint for the social, political, and economic development of an independent Republic."

This may not convince everyone and certainly not the exiles in Miami. But Kirk has managed to make clear the view that Martí did indeed have a set of readily recognizable revolutionary ideas. Moreover, the book would seem to undercut Richard Butler Gray's much-discussed thesis (in *José Martí: Cuban Patriot* [1962]) that Martí, essentially the journalist, did not have a definable corps of political thought. The Kirk volume needs to be weighed against the Gray book, which is the most recent previous attempt to explain Martí in English. Both have their points. But this reviewer is persuaded that the Kirk thesis moves us closer to an understanding of Martí. The Kirk volume, moreover, includes an extensive bibliography, detailed notes, and a useful chronology.

JAMES NELSON GOODSSELL
Christian Science Monitor

BRUCE J. CALDER. *The Impact of Intervention: The Dominican Republic during the U.S. Occupation of 1916–1924*. (Texas Pan American Series.) Austin: University of Texas Press. 1984. Pp. xxxii, 334. \$22.50.

Bruce J. Calder's study of the United States intervention in the Dominican Republic from 1916 to

1924 is one of those books that combines solid historical research with valuable insights into an era when this country felt a responsibility to bring order and reform to many Caribbean and Central American nations. The strength of Calder's book lies in its thorough examination of the administration, policies, reform initiatives, and antiguerrilla actions of the United States military government. But the author's critical evaluation of this eight-year period, his sensitivity to the impact of the intervention on the Dominican people, and his ability to capture the internal "climate" that is created when a major power forces its will on another country transform this book into a work of exceptional historical analysis.

Perhaps the most important contribution of Calder's study is its putting to rest the commonly held view that the American intervention had a positive effect on Dominican society. Although Calder shows that the United States military governors instituted a wide array of reforms in areas such as education, sanitation, public works, taxation, and property rights, many of these changes were short-lived because of fiscal constraints or were simply ignored by a populace unwilling to accept the dictates of an occupying army.

The most interesting aspect of the book is his description of the guerrilla movements that sprang up in response to the United States intervention. The rebellion of the Dominican people, especially in the eastern sugar provinces of the country, has long remained a mystery. Using extensive data from military documents and personal correspondence, Calder draws the first clear picture of the war waged against the Marines. The presentation of the counterinsurgency operation by the American military units in the eastern provinces is a sad commentary on the incompetence, bigotry, and cruelty of the occupation forces. The Marine contingents were woefully inexperienced in guerrilla warfare and never really attained control of these valuable sugar-producing lands. Their frustration at not being able to capture the elusive guerrilla leaders often led to harsh treatment of the civilians who supported the uprisings. When guerrillas were taken prisoner, Calder shows that the American military commanders resorted on occasion to torture, which only intensified the opposition. In the end it was more the prospect of the withdrawal of the United States that pacified the rebels than any military "victory" of the Marines.

The United States eventually did leave the Dominican Republic, but the process of transferring power back to the Dominican people was not accomplished without bitter debate. Nationalist forces in the country pressed for an unconditional withdrawal, often using the phrase *pura y simple*, while more moderate politicians were willing to accept

certain United States requests (such as a provisional government and a new loan arrangement). The United States succeeded in forcing its withdrawal program on the Dominicans but not before the Nationalists were able to mount a vigorous opposition campaign. As Calder points out, one of the enduring legacies of the American intervention was the sense of nationalism that developed in the Dominican Republic during the negotiations for the withdrawal.

After any period in which one country dominates another there is the natural desire among historians and political analysts to critique the performance of the intervenor and comment on the benefits and drawbacks of the intervention. Calder is not shy about evaluating governance of the Dominican Republic by the United States during the 1916-24 period. He calls our intervention "neither wise nor just" and our policies "basically unproductive." Calder's conclusion is that this episode in American imperialism was an unfortunate waste of our men and resources.

Calder is to be commended for his forthright analysis of the American occupation, but I was a little disappointed at his failure to link the intervention with the future direction of Dominican politics. Although Calder thoroughly describes the formation of the National Guard by the United States as a police force designed to maintain order once the Marines left, he only briefly mentions Rafael Trujillo and his use of the National Guard to establish Latin America's most repressive and longest running authoritarian regime. Even though Trujillo did not assume power in the country until 1930, it is important to show that, besides bringing order and instituting reforms for eight years in the Dominican Republic, the American occupation also laid the groundwork for a system of government that was the very antithesis of Woodrow Wilson's original plan for "civilizing" these neighboring nations.

Despite the failure to connect adequately the intervention to the rise of Trujilloism, Calder's book is a welcome addition to the growing volume of research on this important period of American foreign policy. Moreover, with the United States currently in the midst of a debate over the future direction of its policy in Central America, *The Impact of Intervention* will help to alert readers to the pitfalls of flexing our military might and directing the internal affairs of those nations closest to our borders.

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MARK WASSERMAN. *Capitalists, Caciques, and Revolution: The Native Elite and Foreign Enterprise in Chihuahua, Mexico, 1854-1911*. Chapel Hill: University of North Carolina Press. 1984. Pp. xii, 232. \$27.00.

For nearly two decades, an increasing number of scholars have been immersing themselves in the study of Latin American history on a regional scale. They have probed from a great variety of perspectives: dependency, elite hegemony, entrepreneurial activity, occupational patterns, family connections, labor systems, and export-led growth, to name the most important. The difficulty has come in orientation to the complexities of the regional level of historical experience. Regional historians have uncovered storehouses of detail and provided challenging interpretations, but, at the same time, they have been too taken with their immediate finds to explore around the next bend of the region's multifaceted landscape.

Mark Wasserman has been far more willing than most to continue his explorations. His research on Chihuahua during the Porfiriato has evolved from a regional elite study to a complex interpretation of the Porfirian regime at the state level. It takes into account dependency and revolutionary theories, family and national-regional-district connections, class and group conflict, and interregional comparison. The great value of *Capitalists, Caciques, and Revolution* for regional scholars lies in this complex conceptual framework and the extensive quantitative data and detail that accompanies it. In these very strengths, however, one should also find lessons of caution.

Wasserman's study focuses on three closely related historical phenomena: the rise to regional hegemony of a single family (the Terrazas) and its clients; an export-led economy primed by foreign markets and investment; and the discontent created by the preceding two, which led in 1910 to revolution and in an alliance of the middle class, peasants, and workers. Family connections also enter extensively into the author's thesis. Wasserman's data on the economic activities of the Terrazas (and foreign entrepreneurs to a lesser extent) are at times overwhelming in their scope, a testament to his range of sources. His extensive use of other regional experiences, rare in such monographs, is to be lauded. His explanation for the decade-long ferment of socioeconomic groups that erupted in revolution is fresh, well documented, and compelling.

Nevertheless, it is Wasserman's frequently inadequate integrative analysis—the problem of weaving together such a complex conceptual framework and such a range of detail—that points to an even more difficult challenge for regional historians of Latin America. In this book there is far too much listing of factors and grouping of details, as evidenced by the many brief subsections and numerous redundancies. More particularly, his treatment of the revolutionary alliance of middle class, peasants, and workers is largely an isolated discussion of each opposition group. In addition, the bulk of evidence is

weighted toward the decade prior to the revolution, even though the thesis spans a half-century. Another problem is the lack of satisfactory definitions of elite, middle class, and occupation (vis-à-vis class). Consideration of family connections, such as poor relations, and client ties might have provided clarification. The family tree in the appendix could have provided rich evidence for further exploration.

Wasserman's research on Chihuahua demonstrates what can be achieved in a regional study of Latin America, if scholars are willing to probe deeply and broadly enough. It also manifests the need for even more sophisticated analysis.

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MARY KAY VAUGHAN. *The State, Education, and Social Class in Mexico, 1880–1928*. (Origins of Modern Mexico.) DeKalb: Northern Illinois University Press. 1982. Pp. x, 316. \$22.50.

This considered study traces the evolution of public education in Mexico from the Porfiriato, with its positivistic philosophic expression, through the more structured cultural nationalism of the Obregón and Calles periods. Mary Kay Vaughan focuses on the continuity that underlies educational thought during the prerevolutionary and postrevolutionary periods. In doing so, the study demonstrates how the Mexican state traditionally has used education as a means of social control.

The author examines those figures who shaped national educational policy from Justo Sierra through José Vasconcelos. The Mexican Revolution of 1910 is interpreted as one in which the developing capitalism of the Porfiriato was furthered by the kaleidoscopic events of the times. Vaughan's perspective of the revolution is that of a "national bourgeois revolution" (p. 7), one in which nationalistic economic and developmental policies benefiting the middle class took precedence over those favoring the working class and the peasantry. Although foreign capital remained dominant, the Mexican middle class emerged triumphant. The author believes this to be basic for understanding the ideology-shaping educational policies in Mexico during the period under study.

Revolutionary figures such as Felix Palavicini decried the elitism inherent in positivism and called for programs dealing with social reform. Yet his real fear was the threat the uneducated masses posed to property and the middle class. Education was seen more as a means to prevent social unrest than as an effective way to bring the masses into the larger society. Moreover, educators believed that the acqui-

sition of basic skills by the masses was all that was required to free them from continued exploitation by those who held the power; few advocated basic structural change as necessary if the masses were to be integrated effectively into the larger society.

The increasing national control over public education, although opposed by revolutionary thought, was made necessary by national realities, for local and state governments had neither the resources nor the sustained will to develop mass systems of public education. The author notes the continued imbalance between urban centers and rural areas of the country. The city remained the favored recipient of monies and talents by prerevolutionary and postrevolutionary governments. Public education was viewed as basic to national development and essential for national integration. Modernization required the acquisition of skills and values by the people.

This is an excellent study in an area where there is little systematic material. Well written and with an extended bibliography, the book is strengthened further by tables and charts illustrating, in more detail, the points under discussion. Included is an analysis of school texts that shows the persistence of Porfirian ideals of law and order, obedience and discipline, and the need to imitate the European experience long after the revolutionary movement emerged. Well organized and analytical in approach, the book provides a valuable understanding of educational thought and pedagogy during a crucial period of Mexican history.

JOSEPH A. ELLIS

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ANN L. CRAIG. *The First Agraristas: An Oral History of a Mexican Agrarian Reform Movement*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xv, 312. \$28.50.

Ann L. Craig has written a rich and evocative study of agrarian reform in a *municipio* in central Mexico. She skillfully weaves a history of the linkages among social classes and political movements at the local, regional, and national levels in the years between 1924 and 1940. Much of this story is told simply in the words of the leaders of the local agrarian movement, for the book combines oral history with extensive documentary research. The study is particularly interesting for its demonstration of the relationship between urban workers and the mobilization of peasant demands for land, its documentation of the importance of external support to the success of local demands for land and justice, and its insights into the importance of local initiative for the implementation of national reform efforts.

Lagos de Moreno, Jalisco, the focus of Craig's work, was not directly involved in the violent phase of the Mexican Revolution. In the first thirty-five years of this century, the *municipio* was characterized by political and social conservatism and by the influence of the Catholic church and of a local oligarchy of landed families. The structure of landholding was diverse, including a limited number of large haciendas, many moderate-sized ones, and an extensive sector of smallholders. A local textile mill was the center where laborers initially organized to demand higher wages and implementation of national labor legislation in the 1920s. From the core leadership of this movement emerged the effort to mobilize peasants to petition state and national governments for the creation of *ejidos*, a movement that reached fruition under the massive implementation of the agrarian reform during the Cárdenas administration (1934–40). Those who led the movement within the *municipio* and at the level of the *ejido* shared a common experience of migration to the United States in the years before the movement emerged. Organization and pursuit of reform objectives entailed considerable risk to those involved: the Cristero revolt, centered near Lagos, pitted agraristas against their conservative neighbors; opposition of the local landed elite was often intense; and alliances with regional and national politicians could prove harmful if the politicians became "losers" in conflicts at state and national levels. Indeed, two from the core leadership of the local struggle were killed as a direct consequence of their agrarian activism. The agraristas who participated "shared a simple ideology linked to the defense of human dignity and moral rights" (p. 8) and held expectations that involvement would bring tangible benefits to themselves, their families, and their group. They eventually achieved these benefits through sustained political activism only to see their initiative gradually incorporated into national political structures.

The results of intensive interviews with participants in the struggles of the 1920s and 1930s, their families, and associates are presented in chapters that explore the motivations and experiences of the core leadership group and the community leadership of specific *ejido* petitioning groups, "la Vieja Guardia." The lives of José Romero Gómez, Macedonio Ayala, Aniceto Martínez, Francisco Navia, Ernesto Rodríguez, Salvador Martínez, Arcadio Amézquita, and others—all humble people who pursued a vision of a more just society for themselves and their families—are sketched with sympathy and dignity. For the student of Mexican politics, *The First Agraristas* provides tantalizing detail and useful analysis about the emergence and decline of a local agrarian movement in the years dominated by Calles and Cárdenas at the national level and about

the gradual consolidation of what was to become the Partido Revolucionario Institucional (PRI). For the student of peasant movements, the book provides exceptional insights into the alliances between peasants and workers and the role of local and supralocal leadership in the mobilization and achievement of political demands. This case study is a model of how local history can illuminate broader processes of change in a society and of the skillful use of oral history to bring human meaning to the causes and effects of those changes.

MERILEE S. GRINDLE
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PABLO OJER. *El Golfo de Venezuela: Una síntesis histórica*. (Biblioteca Corpozulia, number 7.) Maracaibo: Corpozulia. 1983. Pp. 624.

PABLO OJER. *La década fundamental en la controversia de límites entre Venezuela y Colombia, 1881-1891*. (Biblioteca Corpozulia, number 6.) Maracaibo: Corpozulia. 1982. Pp. 618.

Manuscripts totally devoid of methodology and critical analysis are rarely published in the United States, where historians are expected to interpret primary materials. In some places in Latin America it is acceptable simply to extract facts from diplomatic records and compile them in chronological order. The books reviewed here contain a surfeit of factual bricks but no analytical mortar.

Both volumes can be introduced by noting that Venezuela's 1830 constitution provided that the country include all territory of the (pre-1810) colonial Captaincy-General of Venezuela. Neighboring Colombia agreed to that provision in an 1833 treaty, which neglected to fix some common boundary points. Border clashes ensued, and relations between the nations were ruptured in 1873 and 1875. In 1881 the countries agreed to let Alfonso XII of Spain arbitrate their border differences. He died suddenly, and a decade later Spain's queen regent rendered a decision. But mutual distrust, economic rivalry, Indian raids, and internal strife precluded accord. The potentially oil-rich continental shelf under the Gulf of Venezuela remained the major source of conflict between the states.

La década fundamental deals with the diplomacy of 1881-91 and is based on archival records from Madrid, Seville, Caracas, and Bogota. Pablo Ojer transformed thousands of documents into prose and has produced more of a legal encyclopedia than a diplomatic history. For most historians, Gordon Ireland's standard work, *Boundaries, Possessions, and Conflicts in South America* (1938), provides sufficient information about, and deeper insights into, the period under scrutiny.

El Golfo de Venezuela examines the jurisdictional disputes between Colombia and Venezuela over the Gulf of Venezuela and the Guajira Peninsula and its surrounding islets. The book is predicated on Venezuela's adherence to the theory of *uti possidetis juris*, the idea that a treaty between parties vests in them the territory under their actual control, and the principle that natural limits determine legal boundaries. The author notes every land transfer, territorial incursion, and diplomatic incident in the contested zone from the arrival of the Spanish to the present. He documents the diplomacy of the 1890s, the work of the eight major boundary commissions convened between 1894 and 1918, and the inconclusive 1922 Swiss arbitration.

A 1941 treaty between the countries, designed to maintain hemispheric solidarity during World War II, ostensibly terminated all boundary problems. Colombia then assumed control over a group of uninhabited rocks in the Guajira Peninsula known as Los Monjes, but in 1952 Venezuela declared sovereignty over the area, which reportedly contained uranium and oil. Colombia agreed to Venezuela's sovereignty claim. But the countries disagreed on their maritime boundary and control over the water around Los Monjes. Secret negotiations about the conflict ended in 1973 when Colombia tried to take the matter to the International Court of Justice. Venezuela, fearful that submitting to arbitration would be interpreted as a cession of sovereignty, refused to arbitrate. Currently, Colombia wants to reduce oil imports by extracting petroleum from the disputed zone. The area also has increased potential value for Venezuela, which has derived greater profits from its recently (1976) nationalized oil industry.

Both books are at best references, collections of geographical and diplomatic data containing a few inadequate maps. Neither volume examines the strategic military, commercial, or social significance of the regions in dispute. Nor do the books provide perspective on the philosophical legal implications or the international political aspects of the boundary problems in question.

The author indirectly corroborates the reviewer's contention, printed elsewhere, that an inordinate amount of Venezuelan and Colombian diplomacy, especially during the nineteenth century, has pertained to territorial disputes, and confirms his belief that domestic political chaos has adversely affected the nations' ability to settle international controversies. Venezuela's Ministry of Foreign Relations supported publication of both books, which tend to reflect the ministry's points of view.

SHELDON B. LISS
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LARS SCHULTZ. *The Populist Challenge: Argentine Electoral Behavior in the Postwar Era*. (James Sprunt Studies in History and Political Science, number 58.) Chapel Hill: University of North Carolina Press. 1983. Pp. xiii, 141. \$9.95.

On October 30, 1983, Argentine voters went to the polls for the first time in more than ten years to choose a new president. Confounding the expectations of many, they handed the candidate of the Peronist party a resounding defeat and invested their hopes in Raúl Alfonsín, a veteran politician from the Radical party. It was the first time ever that the Peronists had lost a presidential election in which they were permitted to participate freely.

The results of the 1983 elections were but an extension of developments that began with the sudden collapse of the military regime that had governed Argentina since 1976, and they provide a convenient touchstone by which to test Lars Schoultz's timely monograph on Argentine voting behavior. Although *The Populist Challenge* was written before the elections, its major finding holds up remarkably well.

Schoultz characterizes postwar politics in Argentina as a struggle between a multiclass populist coalition, united under the banner of Peronism and the supporters of economic liberalism, advocates of a free-market system congenial to international influences. The latter have consistently failed to provide able leadership during turns at the helm achieved as a result of military interventions. This was especially true of the most recent junta, which compiled an atrocious record of human-rights violations, lost an ill-advised war against Great Britain in the South Atlantic, and left the Argentine economy in shambles. According to Schoultz, the future belongs to the populist movement, which will ultimately prevail over the economic liberals. He sets out to examine the nature of Peronist electoral

strength in order to evaluate the prospects for long-term stability in Argentina.

Using data from elections held between 1946 and 1974, Schoultz first measures the impact of three variables—demographic change, industrial development, and economic satisfaction—on support for Peronist candidates. The author concludes that demographic change and economic satisfaction have had an insignificant effect on Peronist vote totals and that industrial growth provides a more reliable measure of Peronist strength than an area's absolute level of industrialization. Schoultz then examines the nature of changing patterns exhibited by Peronist voters from election to election and determines that areas evidencing the highest levels of support for Peronism also exhibit the most voter fluidity.

The most dramatic implications Schoultz draws from this analysis are that Peronism suffers from much more voter defection than the anti-Peronist parties and that the death of Juan Perón will exacerbate this tendency. The results of the 1983 election, in which surprisingly large numbers of working-class voters cast their ballots for the candidate of the Radical party, have borne out this prediction.

Yet the triumph of the Radicals raises questions about the populist-liberal dichotomy underlying Schoultz's analysis. If Alfonsín succeeds in wresting the populist mantle away from the Peronists or in forging a new postpopulist movement, he will render much of Schoultz's work of historical interest only, since Alfonsín will be leading a new coalition with its own dynamics. If not, this book will be of value in assessing Peronist chances in future electoral contests, although the emergence of the Radical party as a powerful force independent of Peronist populism and economic liberalism will serve to complicate Argentine politics even further.

JOSEPH A. PAGE
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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ALAN R.H. BAKER and DEREK GREGORY, editors. *Explorations in Historical Geography: Interpretive Essays*. (Cambridge Studies in Historical Geography, number 5.) New York: Cambridge University Press. 1984. Pp. vii, 252. \$49.50.

ALAN R.H. BAKER, Reflections on the Relations of Historical Geography and the *Annales* School of History. MARK BILLINGE, Hegemony, Class, and Power in Late Georgian and Early Victorian England: Towards a Cultural Geography. DEREK GREGORY, Contours of Crisis? Sketches for a Geography of Class Struggle in the Early Industrial Revolution in England. MARK OVERTON, Agricultural Revolution? Development of the Agrarian Economy in Early Modern England. RICHARD M. SMITH, "Modernization" and the Corporate Medieval Village Community in England: Some Special Reflections. ALAN R. H. BAKER and DEREK GREGORY, Some *terrae incognitae* in Historical Geography: An Exploratory Discussion.

RUDOLPH BINION. *Soundings: Psychohistorical and Psycholiterary*. New York: Psychohistory. 1981. Pp. v, 164. Cloth \$18.95, paper \$18.95.

What the Metamorphosis Means. Repeat Performance: Leopold III and Belgian Neutrality. From Mayerling to Sarajevo. Hitler looks East. Doing Psychohistory. The Play as Replay or the Key to Pirandello's *Six Characters in Search of an Author*, *Henry IV*, and *Clothe the Naked*.

BARBARA J. HARRIS and JOANN K. MCNAMARA, editors. *Women and the Structure of Society: Selected Research from the Fifth Berkshire Conference on the History of Women*. (Duke Press Policy Studies.) Durham, N. C.: Duke University Press. 1984. Pp. xi, 305. Cloth \$37.50, paper \$14.75.

BOYDENA R. WILSON, Glimpses of Muslim Urban Women in Classical Islam. JEAN E. FRIEDMAN, Piety and Kin: The Limits of Antebellum Southern Women's Reform. GWEN VICTOR GAMPEL, The Planter's Wife Revisited: Women, Equity, Law, and the Chancery Court in Seventeenth-Century Maryland. JOEL T. ROSENTHAL, Aristocratic Widows in Fifteenth-Century England. JOYCE ANTLER, Was She a Good Mother? Some Thoughts on a New Issue for Feminist Biography. ANN B. SHTEIR, Linnaeus's Daughters: Women and British Botany. JODI BILINKOFF, The Holy Woman and the Urban Community in Sixteenth-Century Avila. SANDRA F. MCGEE, Right-Wing Female Activists in Buenos Aires, 1900-1932. FRANCOISE DUCROCQ, The London Biblewomen and Nurses Mission, 1857-1880: Class Relations/Women's Relations. JOAN JACOBS BRUMBERG, The Ethnological Mirror: American Evangelical Women and their Heathen Sisters, 1870-1910. JEAN M. HUMEZ, "My Spirit Eye": Some Functions of Spiritual and Visionary Experience in the Lives of Five Black Women Preachers, 1810-1880. GAYLE GULLETT, City Mothers, City Daughters, and the Dance Hall Girls: The Limits of Female Political Power in San Francisco, 1913. ELLEN DUBOIS, The Limitations of Sisterhood: Elizabeth Cady Stanton and Division in the American Suffrage Movement, 1875-1902. REGINA MARKELL MORANTZ, Feminism, Professionalism, and Germs: A Study of the Thought of Mary Putnam Jacobi and Elizabeth Blackwell. ELINOR LERNER, Jewish Involvement in the New York City Woman Suffrage Movement. ELDA GENTILI ZAPPI, "If Eight Hours Seem Few To You. . .": Women Workers' Strikes in Italian Rice Fields, 1901-1906. HARRIET B. APPLEWHITE and DARLINE GAY LEVY, Responses to the Political Activism of Women of the People in Revolutionary Paris, 1789-1793. MARIAM DARCE FRENIER, The Effects of the Chinese Revolution on Women and Their Families.

UNIWEYSYTET JAGIELLONSKI. *Emigration from Northern, Central, and Southern Europe: Theoretical and Methodological Principles of Research*. (International Symposium, Cracow, November 9-11, 1981.) Cracow: The University. 1983. Pp. 348. 175 Z.

JULIANNA PUSKÁS, The Process of Overseas Migration from East-Central Europe: Its Periods, Cycles, and Characteristics, a Comparative Study. JAN TUROWSKI, Theory of Integration and Disintegration as the Adequate Theoretical Framework for Ethnic Studies. WOLFGANG HELBICH, Prob-

lems of Editing and Interpreting Immigrant Letters. KEIJO VIRTANEN, Oral History and the Study of Finnish Overseas Migration. MATJAŽ CLEMENČIČ, Methodological Questions on the Reliability of American Statistics of Mother tongue for Immigrants from Austria-Hungary. ANDRZEJ BROŻEK, Selected Methodological Problems Found in the Literature on the Polish Ethnic Group in the United States. HANS NORMAN, Swedish and Scandinavian Emigration to America: Some Findings from the Work of the Uppsala Group. KRZYSZTOF GRONIEWSKI, Emigration from Poland to America: A Historian's View. EDWARD KOŁODZIEJ, Emigration from II Polish Republic to America on the Background of Employment Seeking Emigration Process from Poland: Number and Structures. JOSEF V. POLIŠENSKÝ, Problems of Studying the History of Czech Mass Emigration to the Americas. MARK STOLARIK, Slovak Emigration to North America in Historical Perspective. HELENA JAKEŠOVÁ, Emigrants from Slovakia: Immigrants to Canada as an Object of Historical Research. IVAN ČIZMIČ, Review of the Research Work and Literature on Croatian Emigration and Emigrants from the 19th Century to World War I. VESNA MIKAČIĆ, The Significance of the Regional Aspect of Emigration: The Example of Yugoslavia. IRA A. GLAZIER, Ships and Passengers in Emigration from Italy to the U.S., 1800–1900. SUNE ÅKERMAN, Migration as a Research Tool and a Study Object per se: Summary of a Text Commenting upon Some Recent Scandinavian Studies. KRZYSZTOF FRYSZTACKI, Migrations of National Groups as an Element of the Urbanization Process. MARCIN KULA, Migrational Movements and Ones of Social Protest. DIRK HOERDER, Migrations in the European and American Economies: Comparative Perspectives on its Impact on Working-Class Consciousness. ANDRZEJ ŚWIATKOWSKI, International Protection of Migrant Workers.

VYTAUTAS KAVOLIS, editor. *Designs of Selfhood*. Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, London. 1984. Pp. 237. \$28.50.

VYTAUTAS KAVOLIS, Histories of Selfhood, Maps of Sociability. PEI-YI WU, Varieties of the Chinese Self. VYTAUTAS KAVOLIS, On the Self-Person Differentiation: Universal Categories of Civilization and Their Diverse Contents. DAVID KOPF, The Idea of Self in the Countertraditions of Bengali Hinduism. ALAN RONALD, The Self in India and America: Toward a Psychoanalysis of Social and Cultural Contexts. SARAH LAWALL, The Self-Person of Surrealism. CORINNE LATHROP GILB, Some Varieties of Metaphor in American Images of Selfhood.

JAMES M. KITTELSON and PAMELA J. TRANSUE, editors. *Rebirth, Reform, and Resilience: Universities in Transition, 1300–1700*. Columbus: Ohio State University Press. 1984. Pp. 367. \$25.00.

JAMES M. KITTELSON, Introduction: The Durability of the Universities of Old Europe. HEIKO A. OBERMAN, University and Society on the Threshold of Modern Times: The German Connection. LEWIS W. SPITZ, The Importance of the Reformation for Universities: Culture and Confession

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ARTICLES

TO THE EDITOR:

I would like to compliment the *AHR* on the selection of a paper about the Paget family by M. Jeanne Peterson ("No Angels in the House: The Victorian Myth and the Paget Women," *AHR*, 89 [1984]: 677-708). It is a contribution not only to women's history, but indirectly to the history of the family and the history of science as well.

I would like to make one minor addition to the article, and then comment upon some enigmatic statements or apparent blindspots in the work. These comments will point to further areas for research, both by Dr. Peterson and by others, including the present writer.

On pages 690 and 694, we are referred to an article for *Macmillan's Magazine* by T. H. Huxley. The translations for the article were done by Catharine Paget around the first three months of 1870. The author states that she was unable to locate the work on which Miss Paget and Professor Huxley collaborated. I would like to suggest with very great probability that the work was Huxley's "On Descartes' Discourse Touching the Method of Using One's Reason Rightly, and of Seeking Scientific Truth," *Macmillan's Magazine*, XXII (May 1870), 69-80. It was the only article that Huxley published with *Macmillan's Magazine* about that time and contained translations from Descartes, Goethe, and French and German historians of philosophy.

Catharine Paget was proud, the *AHR* article states (p. 690), that Huxley was sending the article not to

Nature, but to *Macmillan's Magazine*. This brings up the interesting question, whether the young lady prized belles lettres and a popular audience (and the higher honorarium an article in *Macmillan's* would bring) above the serious scientific scholarship and narrower audience of the new, but later classic *Nature* (founded in 1869). While, on the one hand, Dr. Peterson's point is well taken that the Paget women received good educations and were serious about their learning, the quotation cited, on the other hand, directs us to a less scholarly outlet for their talents as their, and possibly their society's, medium of choice for women. (This reminds me of a Victorian gentlewoman's use of considerable artistic talents in the commercial illustration of greeting cards, or the use of Lydia Paget's musical education [as mentioned in the article] to entertain only her family and guests at home.) This anomaly prompts the larger question, for the Victorian period as well as for our own, whether one's good upbringing and education should be used in a 'professional' role or in a dilettante or amateur status, which represented a higher ideal than that of work and which allowed for greater intellectual independence. In many ways, for the Victorian gentleman and gentlewoman, as for the so-called 'independent scholar' of today, the pursuit of learning was an ennobling avocation, apart from the daily cares of 'business,' law or medicine by which one received one's income. Learning for its own sake, when employed in practice, often involved bringing it down a peg or two.

Several neglected areas might be amplified upon in a future version of the essay. One is the genealogical table (Table 1). It is annoying in traditional genealogical tables to find occasionally that the female offspring and lines of descent have been omitted. By a process of reverse discrimination, this table omits the names of male offspring and marriage partners. Thus one misses possible connections among the women listed and their husbands or their brothers, who may have married (although Dr. Peterson has no record of this) or had other important relationships with females. Also, the female descendants who were not born "Paget" are omitted,

but they are equally as important as the female descendants named Paget, particularly since they descended in the female line (cf. theories of matriarchy and matrilocality, Bachofen, McLennan *et al.*).

Other questions which are touched upon rather briefly, but deserve fuller, more systematic treatment are: (1) the transfer or accumulation of wealth by families by means of marriage (here, for example, the marriage of a wealthier woman to a man from a poorer, intellectual or professional family: Lydia North and James Paget, in Peterson, pp. 680–82, 694–95, 695 n. 58; see also my *The Letter Liveth: The Life, Work and Library of August Friedrich Pott (1802–87)* (Amsterdam and Philadelphia: John Benjamins, 1983), pp. xxxiii–xxxiv), (2) the pattern of residence involved in marriage, i.e., residence in or near the bride's or the groom's family, and (3) the role of (male and female) servants in allowing these upper-middle-class women the time to perform their self-education, intellectual and charitable works (cf. Peterson, p. 707). What sort of atmosphere was produced in the often largely female home by the coterie of largely female servants? Was the atmosphere created one of a harem with a few men in the upper strata or more that of a female preserve, where men were fondly and solicitously cared for, but where they and their outlooks were of comparatively minor concern and where a female world-view (if there be such) predominated?

JOAN LEOPOLD
University of California,
Los Angeles

PROFESSOR PETERSON REPLIES:

I wish to thank Professor Joan Leopold for her kind letter of July 30, and I am pleased to have the opportunity to reply.

(1) Regarding the possibility that Huxley's article "On Descartes' Discourse" was the one on which he and Catharine Paget collaborated: I had, through the medium of the *Wellesley Index to Victorian Periodicals*, come across that same piece and wondered whether it might not have been the article to which Ms. Paget referred. I dismissed it on the ground that, taken together, all her diary references do not point to this sort of piece. She referred three times to translation work in the period January 21–31, 1870: on January 21 she noted that "Papa has looked over our 'gorilla' translation tonight"; on January 28 she referred only to "copying German translations &c."; and on January 31 she referred to *Macmillan* and *Nature* (as quoted in my *AHR* article). Assuming that all these are allusions to her collaborative work with Huxley, then the insistence on translation (as opposed to an article containing translated work) and the allusion to "gorilla" do not

support the idea that the piece on Descartes *et al.* is the answer to the puzzle. I did a search through *Nature* in this period, thinking that Huxley might have changed his mind, but I found nothing more promising than the article Professor Leopold and I uncovered. [I have no idea, by the way, what Catharine meant by the "gorilla" allusion. Given her sense of humor, I thought she (and Huxley) might have been working on a translation having to do with apes, which included, perhaps, some Darwinian connection. I have found nothing to support or refute this possibility.]

(2) Professor Leopold raises the question of what Catharine Paget's preference for *Macmillan* over *Nature* might mean about women's amateurism *vs.* professionalism in their work. In this specific case, I think Catharine rather liked the idea of a wide audience and preferred a well-known periodical to the (relative) obscurity of the new *Nature*. More generally, I believe it is important to avoid making too sharp a distinction between amateur and professional when considering Victorians' attitudes toward scholarly and scientific work. Victorian men and women alike, I think, came to such distinctions only very late in the century, if then. Professor Leopold's point in this respect is well taken. I would put it in rather different language: the blurred line between amateur and professional in Victorian thinking reminds us that work had a different relationship to identity for them than it does for us.

(3) Regarding the genealogical table: I used the table as a way of giving readers some guide to the "players," and I regret that any reader should have found it irritating. The men of the Paget family can, of course, be found in any edition of *Burke's Peerage and Baronetage*, and hence their absence from the chart would be, I thought, only a minor inconvenience to readers, pending publication of my book on the Paget men's careers. Females not born Paget were omitted from the chart because, simply, there were none in the first three generations. Martha and Katherine never married, and the other daughters of Sam and Betsy died in infancy or childhood. I left out the names of women in the fourth generation because they were not included in the study. I fully agree with Professor Leopold about the importance of female connections in understanding the patterns of social and professional relationships of the family as a whole. Those connections will be part of the story I will tell in my book on the Paget men.

(4) Let me respond briefly to Professor Leopold's other questions. The place of residence for these couples seems determined largely by the husband's employment, in terms of both city and neighborhood. (Of course, that employment may have been influenced by family ties, but that is another story.) The atmosphere of most of the households was, as

far as I can tell, remarkably free of the "harem"-like aura that Leopold suggests. They were bustling households, with adults and young people coming and going, with many visitors of both sexes, and with much free exchange, including (by the mid-Victorian years) plenty of argument and intellectual exchange between males and females. The outside world of men's professions was very much a part of the women's knowledge and concern.

Finally, I must confess that in the process of writing this article and in my research and writing about the Paget men, I have become increasingly aware of the positive roles that women played in Victorian society, both in the work they chose to do and in their involvements in their husbands' careers. The Paget women and their friends were not just not angels, they were busy, involved, contributing members of society. The result is a nearly completed book that takes the story beyond the Paget women to look at their wide circle of female relatives and friends, their intellectual characteristics, and their work.

M. JEANNE PETERSON
Indiana University

town and focuses on significant events that took place in the twentieth century. Although the book is 340 pages in length, Lida devotes 19 lines of a 69 line review to the footnote on page 24, in which I briefly disputed the authenticity of the manuscript in question. She did not reveal, furthermore, that the footnote concerned her own work. The remainder of the review is devoted to an attempt to raise questions about my judgment, critical standards, and proofreading ability. That I believe Lida was duped by a patent forgery is a minor issue open to debate. It is of greater significance that Lida's pique appears to have made her incapable of rendering an unbiased and informative review.

I hope that readers interested in peasant revolution, anarchism, oral history, and Spain will take the opportunity to examine my book for themselves. Other reviews of the book can be seen in the *New York Review of Books* (September 23, 1982) by Raymond Carr, the *American Ethnologist* (February 1984) by David Gilmore, and the *Journal of Modern History* (December 1983) by Stanley G. Payne.

JEROME R. MINTZ
Indiana University

REVIEWS OF BOOKS

TO THE EDITOR:

In her review of my book, *The Anarchists of Casas Viejas* (University of Chicago, 1982, paperback 1983), Clara E. Lida fails to inform the reader that she has a personal stake in the subject, related to her own work on anarchism (see *AHR*, 88 [1983]: 1276-77). Her failure to acknowledge her place in the discussion leaves doubt regarding her professional objectivity.

In 1969 Lida published a nineteenth-century manuscript from the Jerez archives. It purported to describe the oaths and duties of the members of a secret society of anarchists accused of plotting the murder of local landowners. The document had been used by the police and prosecution as evidence against vineyard workers accused of membership in this alleged terrorist organization. At a subsequent meeting, I pointed out to Lida why I believed the manuscript to be a forgery used by the authorities to arouse public opinion and justify the brutal repression that followed. Shortly afterwards, I sent her a detailed statement pointing out where I believe she erred in accepting the manuscript as genuine. She had ample time to respond to my views but did not. In the book I limited my objections to a footnote since the matter was only of peripheral interest.

My work on Casas Viejas is an ethnohistory of the

PROFESSOR LIDA REPLIES:

My criticism of Jerome Mintz's book was scholarly not personal, based on its author's problems with historical interpretation and method. While I praised his contributions to Andalusian ethnography and to the study of the 1933 revolt, I questioned his (1) frequent inconsistencies, contradictions and disregard for facts, (2) lack of conceptual clarity, (3) uncritical statements, (4) careless scholarship. In my review I substantiated these objections briefly; here I'll expand them through other major examples.

(1) Mintz repeatedly contradicts the sources he considers most reliable: the oral memoirs by witnesses of the revolt who, according to him, though "uneducated and often illiterate" (IX), have their voices to record their true history. Of the approximately 37 participants, identified by name, Mintz interviewed some 28 survivors. Their testimonies directly reveal that among them 14 could read and write, 3 could not (11 are not described), while among the total 37 participants, 25 were literate, 3 illiterate, and 9 undefined. In sum, roughly 8 to 11 percent illiterate, 50 to 68 percent literate, and 24 to 39 percent unclear (although many participated in *sindicato* activities requiring some knowledge of the three R's). These are the villagers Mintz portrays as a "vast majority" of illiterate laborers and leaders (80; 19, 260, *passim*). Take, for example, Seisdedos, the alleged leader whom Mintz clears of responsibility. He appears as an "apolitical," uneducated, "ordinary man" (275, 164), despite one witness recalling

in a letter to a newspaper (is this illiteracy?) that the old man (whose age varies from p. 163 to 199) read "whatever fell into the hands of his large family," be it anarchist novels or newspapers (275n). *Bref*, are we to rely on the author or his sources?

(2) These contradictions underlie several problems. Among them, (a) a conceptual confusion which equates *education* and *literacy*, leading to the belief that only formal schooling at an early age produces literate and educated individuals. In contrast, those who "had never been to school" Mintz believes "could not read," and remained ignorant (80; 64, *passim*). He underestimates the far reaching implications that alternate schooling by anarchist organizations had since the nineteenth century in raising literacy and consciousness among the Spanish poor. In an illiterate society, literate peasants were a small minority, but within it anarchists stood out. (b) By aggrandizing the magnitude of illiteracy, Mintz endows his method with supreme legitimacy: oral history becomes uniquely superior to the written (IX, X). Could this explain why, regardless of their overwhelming testimonies as to their literacy, anarchists are obstinately described as "a league of illiterate men" (84)?

(3) Mintz, who is not a professional historian, discusses historical events—other than those of Casas Viejas—without original sources or exhaustive examination of secondary materials. His inability to deal with broad historical questions explains, for example, his failure to understand that in the nineteenth century like in the twentieth, (a) Andalusian anarchists emphasized self-education and some were literate, not simply peasants who "could not read or even write their initials forward" (24n; 23, 26, *passim*); (b) local anarchist groups were in contact with the wider world and its writings: they often reinterpreted their content, but adopted their language; (c) anarchists were repressed by oligarchical governments which neither guaranteed nor upheld workers' individual or political rights, and never needed to forge complex documentary evidence to legitimize repression. This is what those 19 lines Mintz questions were about: not my work, but his; not one footnote, but his pervasive inability to distinguish between facile uncritical certainties and complex historical inquiry and proof.

(4) I questioned Mintz's scholarly standards after finding some 50 major errors of fact, substance, terminology, translation, and as many minor mistakes: an average of one error per every four pages!

Mintz cites positive reviews from highly respected colleagues. They are not historians of anarchism, and as generalists they praised a compassionate narrative on a compelling subject. The specialist must, however, evaluate the precise quality of the scholarship and question its flaws. To reject this essential part of our professional responsibility leads

to letters such as the one by Mintz, and makes one wonder who is being personal.

CLARA E. LIDA
State University of New York,
Stony Brook and
El Colegio de México

TO THE EDITOR:

David Burner's review of Robert Caro's *Path to Power* (AHR, 88 [1983]: 1346–47) impels me to urge upon historians the correction of an historical mistake. The error, which is in the book and not in the review, is not wholly Mr. Caro's fault, for it is one which has been perpetrated by many others. It is sorely in need of correction.

In August 1941, by a vote of 203 to 202, the House of Representatives passed a bill to lengthen the period of a drafted man's service from one year to two and a half years. Unfortunately, this was described in the press as a "draft extension" bill. Soon, historians of good repute were mistakenly writing that it was a bill to extend the Selective Service Act. Thus, Samuel Eliot Morison called it "an administration bill to extend conscription for the duration" (*The Oxford History of the United States*, 999), and Henry W. Bragdon wrote that "the President recommended that the Selective Service Act be continued for eighteen months" (*The History of a Free People*, 671). So it is not surprising that, later, Mr. Caro wrote that "the Selective Service Act . . . was about to expire. Unless the Act was extended, not only would most of the men in uniform be discharged, but no new ones could be drafted" (*The Path to Power*, 595).

The error keeps getting repeated: now Barbara Tuchman writes that "renewal of the one-year draft law was enacted in Congress by a majority of only one vote" (*The March of Folly*, 31). The dramatic closeness of the vote appeals to historians, if only as an example of the importance of every vote, but it's high time that they stated the issue correctly.

The Selective Service Act was *not* a "one-year draft law." It was *not* "about to expire." It was *not* extended. If the miscalled "draft extension" bill had been defeated, men would have continued to be drafted.

Mr. Caro goes further down the road of error when he writes that, after the bill was passed, "a friend wrote 'the end result [of the bill's passage] was that, when . . . Japanese bombers struck Pearl Harbor less than four months later, the United States had an army of 1,600,000 men instead of a token force of 400,000.'" This is nonsense. If the bill had been defeated, only 19,000 men would have completed their year of service before January 1, 1942. In fact, if the bill had been defeated the army

would probably have been larger on December 7, 1941, than it actually was. In the early fall of 1941, the army announced that all conscripts twenty-six years old or older—about 200,000 men—were being released even though none of them had completed a year's service. This may have been partly because the army's rapid growth was depleting the military supplies needed for lend-lease to Britain and the USSR, but a more obvious reason for it was the discontent in the training camps caused by the passage of the bill.

I concur in Mr. Burner's general opinion of Mr. Caro's book. I write simply in the hope that by publishing this letter, the *American Historical Review* will help historians to avoid repeating an egregious error.

THOMAS H. ELIOT

Member of Congress, 1941–43 and
Chancellor of Washington University, Retired

TO THE EDITOR:

In his surprisingly inept and uninformative (no clear statement of the book's contents is ever given) review of my *Reason of State and Statecraft in Spanish Political Thought, 1595–1640* (AHR, 89 [1984]: 457–58), Professor Michael D. Gordon maintains that I have neglected three “contexts.”

(1) I have presumably dismissed the “historical context that provoked the problem of reason and state and within which this body of thought was elaborated” as “either general knowledge or unimportant.” I do not consider it unimportant, but since I did not write a textbook I assumed that any student of early modern Europe reading the work has, or should have, a competent knowledge of Spanish history.

(2) According to Professor Gordon, the “doctrinal context” is also missing. As proof he offers my “discussion of medical metaphors of the state” and subsequent failure “to explore either the origins of the metaphor in the medieval formulation of the body politic or Machiavelli's use of the metaphor in the *Discorsi*.” Whereas I did examine the possible medical origin of some political terms (notably: *empírico* and *experiencia*), I discussed no metaphors, medical or otherwise, and saw therefore no need to delve into their beginnings.

Professor Gordon, moreover, points out that “the author notes similarities between writers but fails to analyze the possibility of influence.” Considering the number of thinkers studied, the suggested task would have been a prohibitive one in a monograph of this nature, and in any case, both the internal evidence and the present state of our knowledge of Spanish political thought suggest that an analysis of

influence would be, at this stage, but worthless speculation.

Professor Gordon notes that “the perception of a conflict between principle and expediency was not new to the seventeenth century,” and that I do not consider “how the Spanish perception may have been influenced by other attempts to deal with the problem.” True, Italian influence is strong and the French contribution noticeable. But before the impact of Botero and others can be *systematically* and *rigorously* measured, we must understand *precisely* what Spaniards thought—which is exactly the limited objective of my book. The foundation has now been laid on which to set, firmly, Professor Gordon's proposed study.

(3) The “scholarly context” is said to have been neglected, partially because, “in both text and notes, the author avoids mentioning other studies and whether he is in agreement or disagreement with the views expressed in them.” I should not have to remind the reviewer that, with the exception of the writings of Maravall and one or two others, there exists no substantive work in the field of Spanish political thought in the seventeenth century. Indeed, we do not have a single monographic study of reason of state, although the literature abounds in related short studies either too parochial in outlook or excessively prone to arrive at groundless generalizations (to avoid this last serious shortcoming I have indulged in the “many qualifications and clarifications” which Professor Gordon believes lead to “denseness”). I felt, under the circumstances, no obligation to discuss other views; in fact, Maravall's case excepted, there are no views with which to agree or disagree.

Professor Gordon states that “the author's thesis that the early seventeenth century saw the culmination of a concern with the propriety [of?] reason of state and the beginnings of a concern with the nature of that reason of state is provocative, if not convincing.” Shouldn't a reviewer explain to the reader why the author's thesis is not convincing? I have no idea why Professor Gordon failed to do so, but I do know that on the basis of my thesis as stated above he could not have arrived at a sound explanation. Because although Professor Gordon paraphrases my “nature of that *razón de Estado*” (p. 139), he apparently missed the crucial follow-up: how “nature of reason of state” became in fact “nature of statecraft,” the very title and subject of part II. The culmination of a concern with the propriety of reason of state and the beginnings of a preoccupation with the nature of *statecraft*: such is the true thesis, and only on this basis can the title, structure, and contents of my book be properly judged.

J. A. FERNÁNDEZ-SANTAMARÍA
California State University,
Hayward

PROFESSOR GORDON REPLIES:

Professor Fernández-Santamaría's letter does not deny that he has neglected historical, doctrinal, and scholarly contexts. Rather, it offers explanations for this neglect and these explanations deserve a response.

The author's reason for dismissing the historical context is misleading. The issue is not one of assuming a knowledge of Spanish history; rather, it is one of approach. Ideas are formulated within an historical environment, and a purely doctrinal analysis (pp. xiv–xv) of these ideas which ignores this environment needs to be described as such.

His explanations for neglecting the doctrinal context are disingenuous. He *does* discuss the imagery of the statesman as physician to the body politic (e.g., p. 153), although he characterizes this as a medical analogy (p. 146) and not as a metaphor. Whether it be an analogy or a metaphor, he does not put the use of this imagery into a larger context. The analysis of influence is admittedly difficult; nevertheless, in a study structured around the premise that Machiavelli posed a challenge to later thinkers (pp. 3–5), neglect of the issue of influence merits mention.

The author's explanation that he neglected the scholarly context because no such context exists is—as his own letter suggests—absurd. As the bibliography to his own book clearly indicates, scholars other than J. A. Maravall have examined issues such as reason of state and individuals such as Juan de Mariana.

Some readers of this interchange may well view the author's explanations of why he neglected the historical, doctrinal, and scholarly contexts more positively than I. But surely all would agree that mention of this neglect properly belongs in a scholarly review.

Finally, the author asks me to explain why I found his thesis to be unconvincing and suggests I do not understand that thesis because I paraphrased his "preoccupation with the nature of statecraft" as "concern with the nature of that reason of state." I do not see the significance of this specific point. I did indeed use reason of state as a synonym in this context for statecraft. So does he in both his letter and his book. And so do the political writers who are the subject of his study. That reason of state took on the meaning of statecraft was well known before the author's monograph appeared. Why and how it took on this meaning is the subject of his analysis. And it is on this point that his argument fails to persuade. I arrive at this negative judgment precisely because of Professor Fernández-Santamaría's neglect of the historical, doctrinal, and scholarly contexts. Without the support that such contexts

would offer, his thesis is necessarily less than convincing.

MICHAEL D. GORDON
Denison University

TO THE EDITOR:

In reviewing my book, *Britain's Withdrawal from the Middle East 1947–1971: The Economic and Strategic Imperatives* (AHR, 89 [1984]: 776), J. G. Darwin failed to understand its revisionist thesis. I have made an attempt to demonstrate Britain's decision to withdraw from the most crucial of her possessions in the Middle East: Palestine (1947), the Suez Canal Zone (1954), Aden (1962), and the Persian Gulf (1971). I have argued that Britain's near financial collapse after the Second World War had forced upon her a major retrenchment of imperial and strategic commitments. I have argued that new strategic concepts as an air strike force, atomic capabilities, and new methods of naval warfare contributed to Britain's decision to withdraw.

Dr. Darwin's heavy criticism of my work is a clear indication of a misunderstanding.

(a) The book had to rely mostly on secondary sources because the British archives were not available at the time that the book was written and even if the archives were open I could have used its documents only for the first chapter of my book.

(b) The reviewer argued that there is no systematic discussion of British policy in the Middle East. This book is not, and was not meant to be a survey of British policy in the Middle East. Its subtitle reads: *The Economic and Strategic Imperatives*. These imperatives, as I have made it absolutely clear in my study, were of a domestic nature and acted on the government against any policy which it might have had towards the region.

(c) Dr. Darwin argued that it is astonishing that the Suez Crisis of 1956 was not dealt with. I would like to remind him that the crucial decision to withdraw from the Suez Canal Zone was made in 1954 and not in 1956.

(d) Iran had very little to do with the question of withdrawal from these territories.

I agree with Dr. Darwin that the use of archival sources would have made the first chapter more convincing, but I feel that Dr. Darwin has ignored the important contribution made in the chapters concerning the strategists, the role of air power, the atomic weaponry and the navy. These aspects were not emphasized by the historians who dealt with this subject.

I wonder why Dr. Darwin, who was so diligent about finding faults with this study, failed to find some positive points. Other historians did. To give

two examples, the book reviewer for *Choice* and Joseph Nevo of Haifa in the *Middle East Studies Association Bulletin*.

JACOB ABADI
Fairleigh Dickinson University

ERRATUM

Professor Bernard Sinsheimer has called to my attention a factual error in my review of Richard Cobb's *French and Germans, Germans and French* (*AHR*, 89 [1984]: 141). My review stated that "in 1914-18, only the department of the Nord was occupied. . . ." In fact, the German occupation extended into some adjacent departments as well, though Professor Cobb's book is focused on conditions in the Nord.

GORDON WRIGHT
Stanford University

PROFESSIONAL MATTERS

The following communication is published at the direction of the American Historical Association.

THE EDITOR

It has been brought to my attention that insufficient acknowledgement has been given to the research and the themes of Stephen W. Nissenbaum's "Care-

ful Love: Sylvester Graham and the Emergence of Victorian Sexual Theory in America, 1830-1840" (Ph.D. diss., University of Wisconsin, 1968) in my recently published *Eros and Modernization: Sylvester Graham, Health Reform, and the Origins of Victorian Sexuality in America* (Rutherford, N.J.: Fairleigh Dickinson University Press, 1983). I deplore this inexcusable omission and would like to correct any misconceptions that may arise as a result of my error. I studied closely Professor Nissenbaum's fine dissertation prior to the writing of my own book on the health reform movement. I used some of Nissenbaum's pioneering primary research into the subject as a source guide to my own investigation. Nissenbaum's excellent analysis of the Paris School of Medicine and the careers of Thomas and Mary Nichols were especially consulted in my discussion of the same subject. Moreover, Nissenbaum's contentions that Graham and his followers signified an important intellectual shift in antebellum American attitudes towards sexuality also became a principal theme of my own study. This belated acknowledgement has been put into an *Errata* that accompanies my book, and I urge any reviewers and readers to study the emendations carefully so that Nissenbaum's dissertation can receive the proper credit that was denied it in the original printing. I apologize to Professor Nissenbaum of the University of Massachusetts at Amherst for this lamentable breach of scholarly courtesy and ethics. Students of nineteenth-century American health reform should also be aware that Nissenbaum has published *Sex, Diet, and Debility in Jacksonian America: Sylvester Graham and Health Reform* (Westport, Conn.: Greenwood Press, 1980).

JAYME A. SOKOLOW

American Historical Association

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CORRESPONDENCE: Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

American Historical Review

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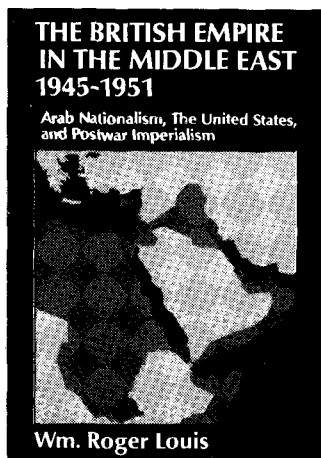
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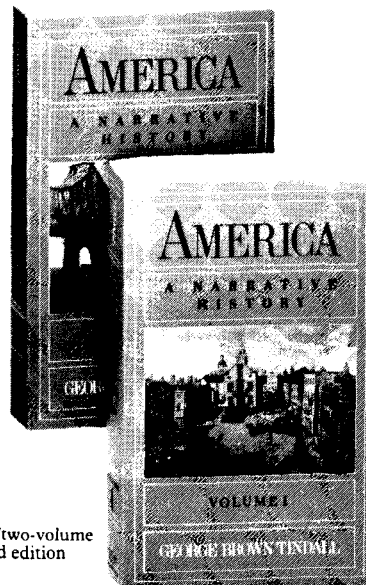
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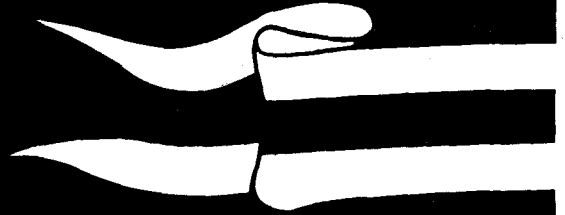
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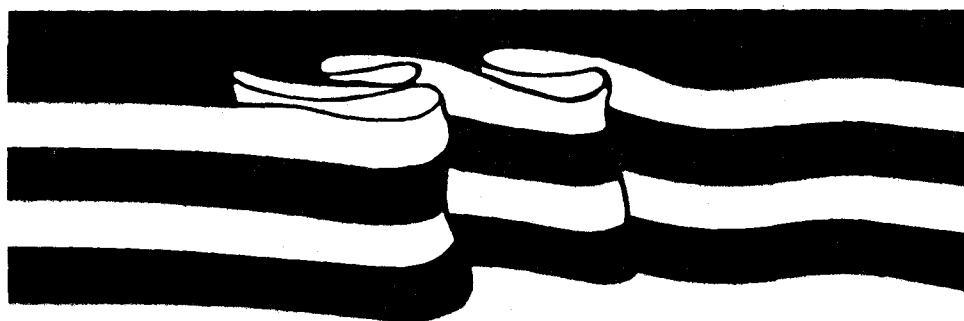
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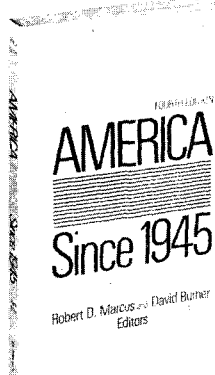
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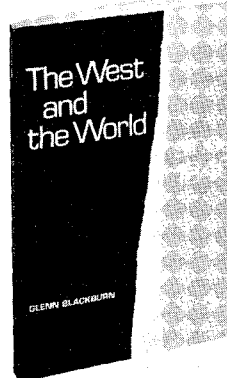
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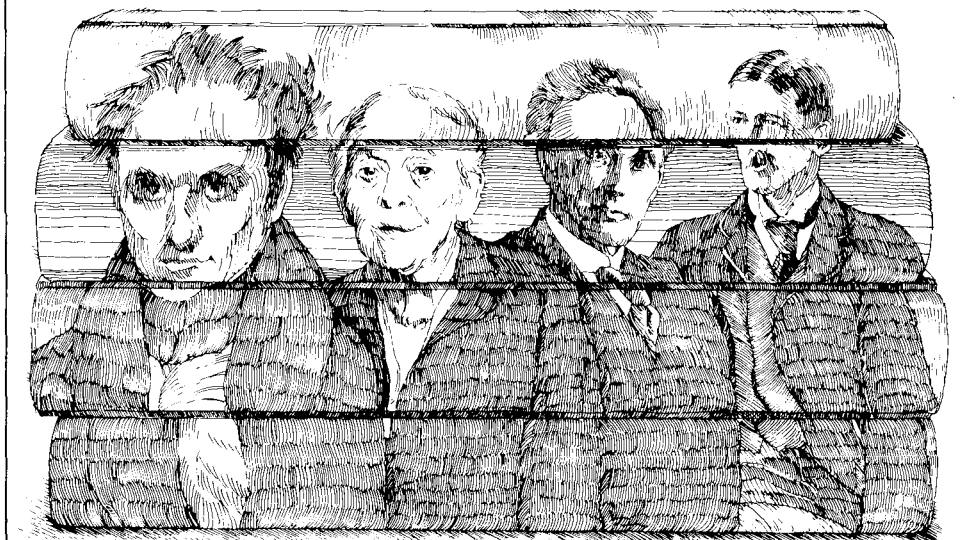
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